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RESEARCH IN OFFICIAL STATISTICS

2003 EDITION

Monographs of official statistics

Papers and proceedings of the colloquium on the history of business-cycle analysis

1

THEME 1
General statistics

EUROPEAN COMMISSION

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Papers and proceedings of the colloquium on the history of business-cycle analysis

Edited by:
Dominique Ladiray

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Introduction

Why a Eurostat colloquium about the history of business-cycle analysis?

Business cycles are always attracting a lot of public attention. The oldest example that I know of comes from the Bible (Genesis 41,29). Joseph had to interpret the Pharaoh's dream and came up with a simple business-cycle model: the total length of the cycle is 14 years, 7 abundant years followed by 7 lean years. In his capacity as political adviser Josef suggested to the Pharaoh not only a theory (at that time yet to be tested empirically), but also a business-cycle policy to cope with the ups and downs. His 'Josephian stabilisation policy' is in stark contrast to what has become 'Keynesian stabilisation policy'. Josef suggested public savings while Keynes proposed public dissavings.

These days, no doubt, our business-cycle models and our stabilisation policies are a little bit more sophisticated. This has also a lot to do with replacing dreams by statistics, of course not so much by agricultural statistics, but by a set of macroeconomic statistics.

We at Eurostat are fully aware that official (and non-official) statistics play an important role in dealing with the business-cycle phenomenon. The business cycle has always been (remember the biblical example) and will continue to be a theoretical construction. The cycle takes shape only through concepts and information, these days less dreams and more statistics. We, the statisticians, are called upon by business-cycle theorists and empiricists to provide the necessary information. Our statistics allow theorists and empiricists to discuss the business-cycle phenomenon in many ways:

- on the basis of reference cycles and diffusion indices derived as synthetic indicators from a large number of leading, concurrent, or lagging series chosen for their supposed relevance for the business cycle;
- on the basis of a macroeconomic aggregate such as gross domestic product or industrial production as deviation from a trend;
- as a degree of capacity utilisation in relation to full capacity compiled either by a full employment rate combined with labour productivity measure or by capital stock combined with capital productivity measure or any combination thereof;

or last, but not least:

- as the result of a univariate or mulitvariate time-series analysis.

Both theorist and empiricist exert considerable influence on policy-makers. They do it directly. Most budgetary forecasts are based on a cyclical analysis. They do it also indirectly. Financial markets are very responsive to business-cycle analysis and policy-makers cannot ignore financial markets these days. The quality of the work of theorist and empiricist is influenced by the quality of their statistical information. Statistics matter and statisticians should be aware of their role.

We here at Eurostat are definitely very much aware of the importance of our role, but we are at the same time also newcomers to the business of compiling regularly according to a strict schedule high-quality statistics for business-cycle analysis. This has to do very much with the profound political and economic changes in the EU. Economic and monetary union is now in full swing, 12 Member States have adopted the euro with notes and coins now introduced. This is why the euro-area business cycle is about to become a phenomenon that is attracting ever more the attention of theorists and empiricists. Euro-zone business-cycle analysis is on, but euro-zone business-cycle statistics have yet to be as good as the national statistics used by business-cycle analysts when looking at the phenomenon from a national angle.

For us, the statisticians, it is very important to have an understanding of the work and of the thinking of the business-cycle analysis community, of theorists and empiricists alike. Only a good understanding of their needs will allow us to suggest relevant statistics and prepare them adequately. Such an understanding should, of course, not be confined to current practices. A look at the history of business-cycle analysis is of particular importance as this field is under permanent change. Modern business-cycle analysis started with synthetic indicators, then came the more economic ones getting ever more sophisticated, now synthetic indicators have become once again more fashionable, albeit with a different content. We have equally seen the rise of time series analysis with each failure having led to a further methodological improvement. Everything seems to go these days, therefore we have to offer something for everybody.

Business-cycle analysts will have to come to grips with the euro-zone cycle and we the EU statisticians have to help them. Some quite difficult questions are awaiting a satisfactory answer in this context. Is the euro-zone cycle best perceived as a condensation of national cycles? Or are national cycles mere variants of a euro-zone cycle? What about lead-lag structures among economies? Consequently there are quite good reasons to look at the history of business-cycle analysis when we are trying to meet the EMU challenge. I am very happy that we were able to bring together a group of historians of renown and hope that EU statisticians, in Eurostat as well as in national statistical institutes, will be able to profit from this colloquium.

Klaus Reeh

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EUROSTAT COLLOQUIUM

HISTORY OF BUSINESS CYCLE ANALYSIS

Luxembourg, 12 November 2001

THE STUDY OF CYCLES AND BUSINESS ANALYSIS IN THE HISTORY OF ECONOMIC THOUGHT

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"In France, sad to say but unfortunately all too well confirmed by our numerous revolutions, our attitude to government is hardly chivalrous. We rely on and applaud it so long as it serves our purposes, or perhaps more accurately, so long as it allows us to serve them ourselves, but as soon as things become difficult and troublesome, most often through our own fault, we cease trusting it. After having taken a wrong turning we call upon government for advice, or – twisting and turning on our bed of pain – demand reforms that are sometimes absurd, which are always regarded as cure-alls and are only a pretext for showing our discontent. The ill-advised and dangerous habit of attributing every good to government during years of prosperity results, by an effect of contrast, in its being accused and blamed for everything that goes wrong at times of crisis. That is the source of the public opinion fluctuations in France, which now exalt a dynasty and now overthrow it. If one were to make a list of governments which, over more than half a century, have survived adversity and intense trade crises, the list would not be a long one".
Clément Juglar, *Des crises commerciales et de leur retour périodique en France, en Angleterre et aux Etats Unis* [*On trade crises and their periodic recurrence in France, England the United States*], 1862.

"Section 2. The Congress hereby declares that it is the continuing policy and responsibility of the Federal Government to use all practicable means consistent with its needs and obligations and other essential considerations of national policy, with the assistance and cooperation of industry, agriculture, labour, and State and local governments, to coordinate and utilise all its plans, functions, and resources for the purpose of creating and maintaining, in a manner calculated to foster and to promote free competitive enterprise and the general welfare, conditions under which there will be afforded useful employment opportunities, including self-employment, for those able, willing, and seeking to work, and to promote maximum employment, production, and purchasing power.

Section 4 (c). It shall be the duty and function of the Council [of economic advisers of the president]… to develop and recommend to the President national economic policies to foster and promote free competitive enterprise, to avoid economic fluctuations or to diminish the effects thereof, and to maintain employment, production, and purchasing power."
Congress of the United States, Employment Act of 1946.

Since the end of the 19th century the relations between theoretical investigation and business analysis have not been intangible. It is not the intention here to present a detailed history of economic thought on the subject, but to propose distinctions sufficient to indicate the successive relationships between economic theory and the practical study of trade. Each of the periods considered corresponds to a specific treatment of the concept of a cycle. As will be seen, the periods in question do not follow a strict chronological succession. Each inaugurates traditions of thinking which are later abandoned and evolve, while overlaps are also quite common.

1. Period I: positivist cohabitation

Until the major crisis of the 1930s there was no clear division between the two types of work, but rather, a reciprocal influence that often became apparent. Theoretical deduction and empirical induction cohabited in some harmony under the aegis of a positivist view of historical events.

This tradition is rooted in the 19th century. From 1862, Clément Juglar undertook an empirical identification of business cycles and their successive phases, while inquiring into their causes[1]. Although he does not use the concept of a cycle, his emphasis on the systematic recurrence of business crises, on the recurrent mechanism "of their development, explosion and liquidation", justifies the view, held by Schumpeter, that he was the inaugurator of cycle analysis. Juglar attributes to bank credit fluctuations in the form of discounts and advances a major role in generating the cycle, but has little faith in the ability of strict monetary control to reduce its intensity: he expresses scepticism about the *Peel Act* of 1844, which related the issue of money by the Bank of England strictly to its metal reserves. He regards speculative price inflation and the play of the effects of wealth in a society of notables still comparatively few of whom work for their living, as a key factor in propagating excessive expansion which ultimately gives rise to overproduction[2] . When exaggerated development of discounts leads to a decrease in bank metal reserves, crisis is not far away. Once the crisis has struck, the liquidation of goods and an excess of debts open the way to depression. Juglar justifies the importance he attributes to certain factors in the causal explanation of cycles in terms of the statistically observed regularity of their behaviour. In this respect he emphasises the simultaneity of these cyclic manifestations in France and in England since 1800, and the increasing synchronisation with the United States. He quantifies the critical thresholds whose crossing heralds the crisis or, on the contrary, recovery[3] . This indeed outlines the research programme of cycle analysis. In 1880-1890 Juglar's work was to inspire a series of efforts to measure fluctuations "barometrically" by means of a range of indexes sometimes deviating quite crudely from the trend. The purpose of these barometers is to detect symptoms that warn of turning points in economic life, and the medical analogy is often shared by their authors. These attempts have been discussed in the international context at the International Statistics Institute. Armatte (1992) and Vidal (2000) have given a detailed account of them, which demonstrates how difficult it is under conditions prevailing at the time to establish barometers that give appropriate weighting to the real and nominal components of fluctuations.

Just after the first world war, the Harvard Institute[4] became the driving force in the United States and the world for the measurement and forecasting of short-term business fluctuations. These were regarded as a specific phenomenon, distinct from the general orientation of economic activity (*the trend*[5]) and from seaso-

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[1] Juglar's founding work, *Des crises commerciales et de leur retour périodique en France, en Angleterre et aux Etats-Unis*, was published in 1862 (Guillaumin, Paris). It appeared in an expanded edition in 1889.

[2] "Increasing luxury brings excessive expenditure, based not on income, but on the nominal estimation of capital according to the quoted share prices." (1862, p. 5).

[3] "It is therefore possible, solely by examining the discount and receipt figures over five or six years, to become aware of how near or far away a crisis is" (1862, p.198). "On examining the main items in the balances of the Banks of France and England, one will have been struck by the regularity, the perfect agreement as it were, that is noticeable between them, despite the complete independence of the two administrations that direct them and the different rules to which they are subject. Whatever their constitution and with the often restricted limits within which one of them, the Bank of England, has to operate, we find the same ups and downs and their periodic recurrence that lead successively to booms and busts of industry and trade" (1862, p. 203).

[4] More exactly, *the Harvard Committee of Economic Research* at Harvard University.

[5] The Anglo-Saxon language distinguishes between the concepts *trend* and *tendency*, which it is difficult not to translate in French by the single word "tendance". The term trend refers more to the long-term direction of economic movement, while *tendency* relates more to the main orientation of that movement in the short term.
Accordingly, the Anglo-Saxon word trend will be used freely, while the French term "tendance" will also be used when the context is not ambiguous, in order not to abuse that anglicism.

nal and irregular fluctuations. They must be extracted from chronological series representing the evolution in time of economic variables using appropriate methods that have recourse to the techniques of moving averages or regression. Such techniques are used to construct the "Harvard barometer" (or *Harvard Index of Business Conditions*), the prototype of a system of cyclic indexes which has been published for the United States since towards the end of the 1910 decade. It consists of three synthetic indices whose respective cyclic movements show a regular correlation staggered by a few months, at least until 1925 (cf. Fig. 1). In periods of expansion, optimism about the future profitability of investments is manifested by a rise in the *speculation index* A (reflecting the evolution of the average share price). It provokes an increase in the demand for products, which leads progressively to price increases and a growth of the business *index* B (which reflects the volume of sales and the price level of goods). As the expansion phase develops, the appearance of tensions in the money market leads to an increase of interest rates manifested as a rise of the *money market index* C (which represents the short-term credit level). However, the weakness of fixed-income values by virtue of the rise in interest rates then affects profitability expectations and leads to a recessive downturn, initiating a process symmetrical to that of expansion.

The figure of Warren M. Persons, who led the Harvard Institute, exemplifies that period. He was a "statistical economist"[6] endowed with profound methodological culture, who paid great attention as much to operational requirements as to theoretical factors. His articles, which give a detailed account of how the Harvard barometer was developed, bear witness to the care devoted to the systematic treatment of the series available in order to group them into a number of categories characterised by similar and synchronised cyclic behaviour: each of the barometer's three synthetic indexes is the product of this process of analysis and classification. Empirical equipment[7] and techniques, new at the time, were put to use for analysing the correlations. The concept of cross-correlation between series showing a shift in time is present. Person's approach is based firmly on an awareness of progress in statistical methodology and information, of which he gives a precise account in an article published in 1925, "Statistics and Economic Theory". For its part, in the 1920s the discipline of economics made good use of the progress in statistical theory achieved in the last decades of the 19th century. That progress naturally led economists, including the greatest among them such as Marshall, who had hitherto remained distant and, rather, cultivated the deductive method, to devote attention to what statistical influence could add to their thinking. The laws of economics, which describe tendencies and are identified by deductive reasoning, do not hold good independently of any perturbing influences (referred to as shocks in today's parlance), which alter their purity and whose discovery justifies recourse to statistical induction. Of course, from the time when statistical economics matured to the point of favouring this combination between theoretical deduction and statistical inference, the range of epistemological positions became broader rather than narrower in relation to the nature and scope of the link that can be envisaged between these two approaches. For Wesley C. Mitchell, who quotes Persons and who will be mentioned again in this review, the emergence of quantitative approaches supersedes older theories too speculative to be corroborated or falsified by a statistical line of argument, and compels theoreticians to acquire a more objective grasp of the behaviour and role of institutions. At the same time, Persons stresses that the study of cycles comes up against still unresolved technical difficulties (statistical characterisation of the periodici-

[6] It is no exaggeration to apply the term to Persons. In his article "Statistics and Economic Theory", *Review of Economic Statistics*, 1925, Vol. vii, he expresses himself as follows: "The term 'economic statistics' may be defined broadly, therefore, to include all the numerical data of mass-phenomena which have an economic application" (p. 179).

[7] Such as the equipment in the form of a screen, called "Illuminated Box with Glazed Top to Facilitate Comparison of Cycle Charts", mentioned by Persons in his very complete article of 1919, "An Index of General Business Conditions", *Review of Economic Statistics*, April 1919, Preliminary Vol. I, which presents the methodology and content of the Harvard barometer. A later article, "An Index of General Business Conditions, 1875-1913", *Review of Economic Statistics*, January 1927, Vol. ix, presents and discusses a "back-extrapolation" of the barometer to the period preceding the first world war.

ties) and brings up theoretical alternatives that are difficult to settle by statistical demonstration alone, in particular because they relate to concepts too vague to be subjected to statistical measurement. The problem of falsifiability, in the sense used by Popper, is implicitly raised.

Since he is aware of these difficulties, Persons attaches great importance to the characterisation of the theoretical debate, to which he devoted an article in 1926, "Theories of business fluctuations", in the *Quarterly Journal of Economics*. Frame 1 shows an extract from that article, which is a listing, a classification of the theories available, without attempting to draw any particular conclusion. In fact, the conception of the three synthetic indexes constituting the Harvard barometer bears the mark of various theoretical references, as several contemporary economists pointed out. It could even be suggested that the cultivated theoretical eclecticism that inspired the designers of the barometer led them to a more synthetic vision of the development of cycles than any of the unilateral theories before them. Fisher alerted them to the role of interest rate, as judged with reference to its normal level, Mitchell about that of realised and expected enterprise profits. In effect it is difficult for a contemporary economist not to approximate the content of the barometer and Wicksell's theory of cycles, based on comparison between the natural or normal interest rate (the anticipated profit rate) and the monetary or nominal interest rate (the remuneration of bank credit)[8]. Yet, Wicksell is ignored by Person's classification.

In constructing his theory of the alternation of inflationary and deflationary phases, Wicksell was directly influenced by the experience of the "Great Depression" in the last quarter of the 19th century, which was characterised in particular by a lastingly high level of real interest rates. The normal interest rate is the rate that would balance the supply and demand for new capital if the lenders and borrowers could negotiate and agree the discounted yield of new productive operations. The economy's engagement in an inflationist or deflationist movement depends on the difference between that normal level and the monetary interest rate effectively applied by the banks in a credit money economy. If innovations that improve the normal rate result in its rising above the monetary rate, the economy will undergo a burst of inflation because of the development of investment (since it will be profitable to borrow from the bank in order to invest) and consumption (since saving in the form of bank deposits will be discouraged by the low monetary creditor rates). This burst will only decline when their balance constraints lead the banks to adjust to the level of the monetary rates: the succession of consecutive expansions of the three indexes A, B and C is re-established. Symmetrical movements bring about a deflationist process. This at least implicit Wicksellian reference nevertheless does not express clearly how the level and variation of production are determined. For that, it would be necessary to wait for the Keynesian view to mature, and that lack was doubtless a factor in the vulnerability of business approaches that mobilise the Wicksellian argument without knowing or saying so.

In this connection it is interesting to note the attitude of the German business analysts of the *Institut für Konjunkturforschung* in Berlin, directed by Ernst Wagemann[9], in the face of the fatal discredit suffered by the Harvard barometer for failing to give a timely warning of the risk of reversal in 1929. Until the Wall Street crash in November 1929, the stability of the advanced index A had seemed to justify the persistence of opti-

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8 Wicksell's terminology was not perfectly fixed, if only because *Interest and Prices*, published in Sweden in 1898, was not translated into English until 1936 (Allen and Unwin, London), and also because it evolved in step with Wicksell's thinking. He specified this, in particular, in his *Lectures on Political Economy* (1901-1906), in two volumes, which were translated into English (Routledge, London) in 1934-1935.

9 In French, one can consult Ernst Wagemann's *Introduction à la théorie du mouvement des affaires*, Paris, Félix Alcan, 1932. This is a summary of his "Konjunkturlehre", Berlin, 1928. Wagemann generalises his approach to the international scale in *Struktur und Rhythmus der Weltwirtschaft*, Verlag Von Raimar Hobbing, Berlin, 1931. An informed and interesting presentation of the German economic analysts can be found in a text by Stéphane Lévy-Valensi, *Ernst Wagemann et Adolph Lowe: l'apogée de la notion de conjoncture*, Miméo, Pierre Mendèds-France University of Grenoble, February 1997.

mistic forecasts (cf. Fig. 1, bottom graph). In 1930 Persons, the creator of the barometer, still believed in a fairly rapid recovery. Although Wagemann notes this fault, he attributes it to the fact that the barometer could not by itself sum up the movement of trade: it is only a barometer of profitability (to use a current term), which has to be included in a broader-ranging system that covers the economic complex more completely. Wagemann proposes a set of three barometers that correspond to the three phases of the capital cycle according to Marx (money-capital, productive capital, commodities-capital): the barometer of profits, which are the source of a new accumulation of capital, combines the indexes already used by the Harvard Institute; the production barometer corresponds to the implementation of means of production; the barometer of outlets reflects the degree of harmony between supply and demand (see Fig. 2). This set also anticipates the Keynesian cycle of demand-production-revenue, if the pole of effective demand and the realisation of profits is stressed more than that of their formation as providing the impetus of the cycle. For Wagemann the economic situation is always "specific" because it interacts with a structure from which it cannot easily be separated.

Thus, at the end of the 1920s German business analysts maintained a balanced attitude between empirical investigation and theoretical awareness. Adolphe Lowe, who collaborated with Wagemann, regarded cycles in his articles of 1926 and 1928 as the result of an endogenous process that upsets the static equilibrium of the economy. He attributes a major role in this process to technological change. He applies intense scrutiny to the relations between economic theory and business analysis: the latter, conscious of the plurality of circumstances, does not fit easily into the logical system of theories based on a static equilibrium concept and interested in the change of that equilibrium when one particular variable is modified, all other things being equal; the phrase ceteris paribus is inappropriate for business analysis, which has to deal with the complexity of the real dynamics of an economic system whose interdependent variables change at the same time but at different rates. From 1933 onwards Lowe took up a university career in England and the United States, where in common with Hicks he developed an analysis of the "cross-link" that aims to illuminate the adjustment processes brought about by a structural change. These processes include the progressive adaptation of the capital stock to a new course of balanced growth.

At the beginning of the 1930s, however, the theoretical-empirical efforts of the German business analysts was left in isolation, due to the force of circumstances. That was not the main direction in which business studies were to move.

Frame 1. Extract from the article by Warren M. Persons, «Theories of business fluctuations», *Quarterly Journal of Economics* , November 1926, Vol. xli, pp. 94-128

In the lists which follow, the letters A, B, C ...are used to distinguish the several classes of theories. The numbers 1,2,3 ...mention the writers. In arranging the writers who belong in the subdivisions, those only to whom chief attention will be given in each subdivision are numbered, others being mentioned in parenthesis.

I - Emphasis on factors other thant economic instituions

A. Periodic agricultural cycles generate periodic economic cycles.
1. W. S. Jevons and H. S. Jevons. 2. H. L. Moore.

B. Uneven expansion in the output of organic and inorganic materials is the cause of the modern crisis.
3. Werner Sombart.

C. A specific disturbance, such as an unusual harvest, the discovery of new mineral deposits, the outbreak of war, invention, or other "accident," may disturb economic equilibrium and set in motion a sequence which, however, will not repeat itself unless another specific disturbance occurs.
4. Thorstein Veblen. 5. Irving Fisher. (A. B. Adams.)

D. Variations in the mind of the business community (affected, of course, by specific economic disturbances) are the dominating cause of trade cycles.
6. A. C. Pigou. (Ellsworth Huntington, M. B. Hexter.)

II - Emphasis on economic instituions

E. Given our economic institutions (particularly capitalistic production and private property) it is their tendency to development which results in business fluctuations.
7. Joseph Schumpeter. 8. Gustav Cassel. 9. E. H. Vogel. (R. E. May, C. F. Bickerdike.)

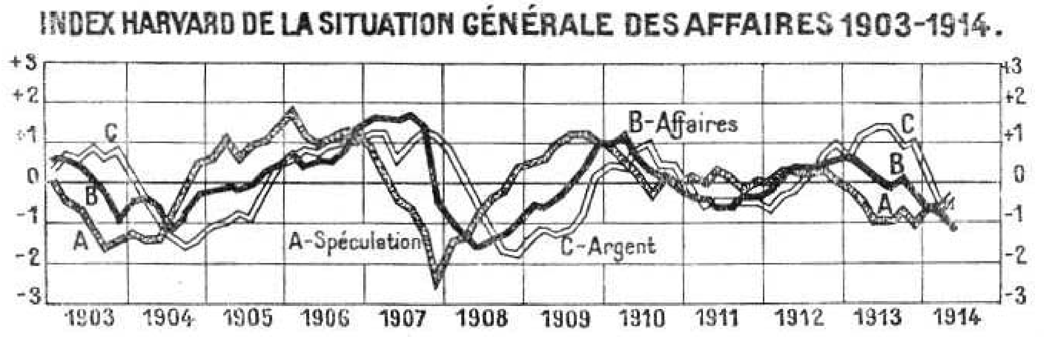
F. The capitalistic or roundabout system of production is the primary cause of business fluctuations.
10. Arthur Spiethoff. 11. D. H. Robertson. 12. Albert Aftalion. (T. E. Burton, G. H. Hull, L. H. Frank, T.W. Mitchcll, J. M. Clark.)

G. Excessive accumulation of capital equipment, accompanied by mal-distribution of income, is responsible for lapses from prosperity to depression.
13. Mentor Bouniatian. 14. Tugan-Baranowsky. 15. John A. Hobson. (M. T. England, W. H. Beveridge, N. Johannsen, E. J. Rich.)

H. The fluctuation of money profits is the centre from which business cycles originate (eclectic theories).
16. W. C. Mitchell. 17. Jean Lescure. (T. N. Carver)

J. The nature of the flow of money and credit, under our present monetary system, is the element responsible for the interruption of business prosperity.
18. R. G. Hawtrey. 19. Major C. H. Douglas. 20. W. T. Foster and Waddill Catchings. (A. H. Hansen, W. C. Schluter, H. B. Hastings, H. Abbati, W. H. Wakinshaw, P. W. Martin, Bilgram and Levy.)

Figure 1 : The Harvard barometer (Key to legends in the top part of Fig. 1):



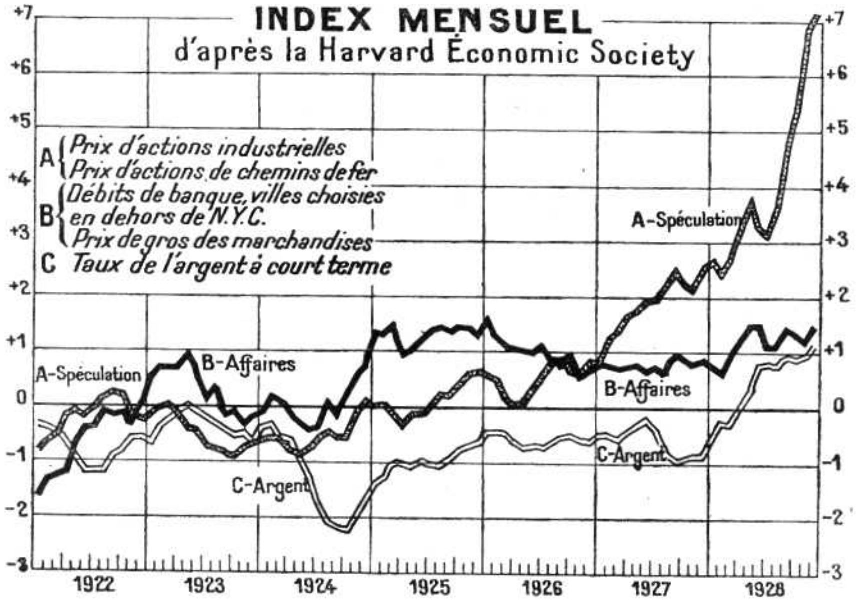
(Key to legends in the top part of Fig. 1):

Harvard index of general business
conditions 1903-1914

A - Speculation

B - Business

C - Money



Key to legends in the bottom part of Fig. 1:

Industrial share prices

Railway share prices

Bank debits, selected towns outside NYC

Wholesale goods prices

Short-term money rates

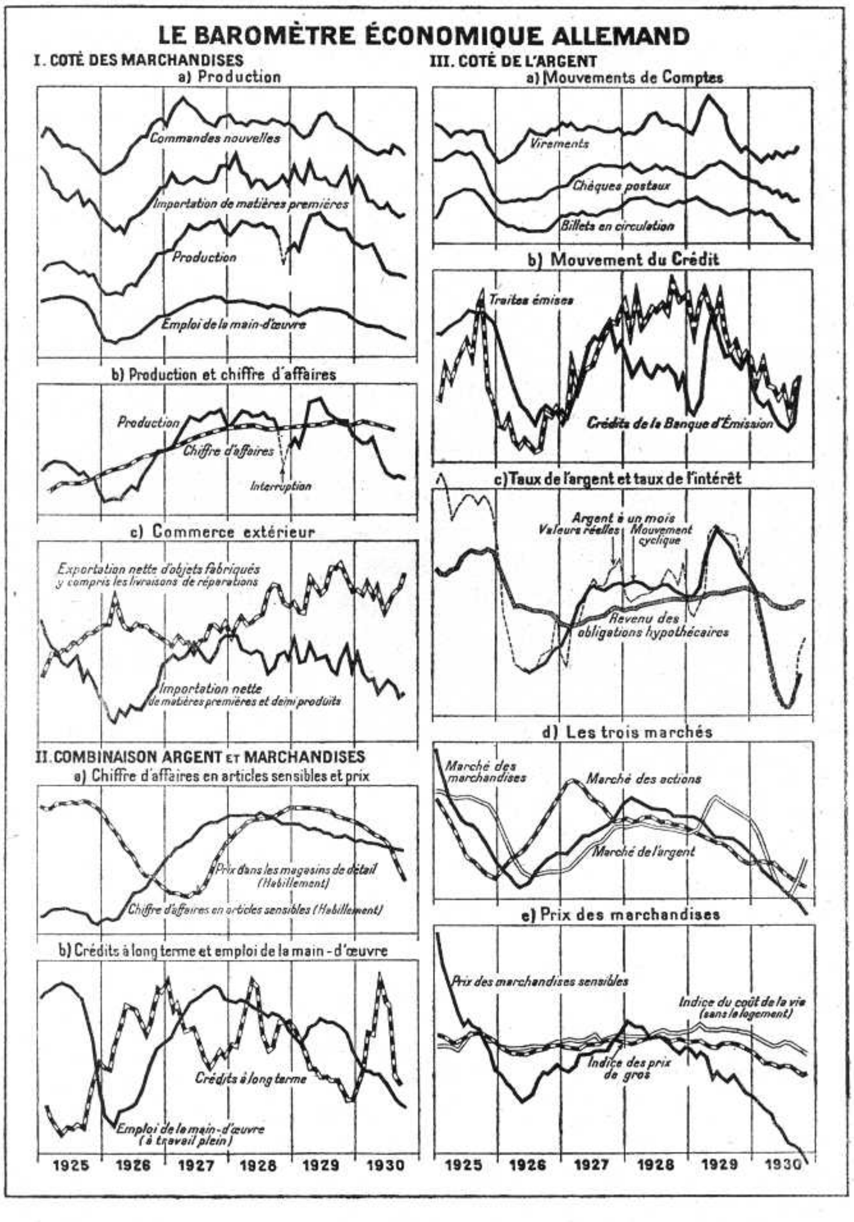
A - Speculation

B - Business

C - Money

Source: Ernst Wagemann (1932)

Figure 2 : The German economic barometer



Source: Ernst Wagemann (1932)

(Key to legends in Fig. 2):

THE GERMAN ECONOMIC BAROMETER

I. Goods Side

a) Production
   New orders
   Raw material imports
   Production
   Workforce employment

b) Production and turnover
   Production
   Turnover
   Interruption

c) External trade
   Net exports of manufactured goods including deliveries of reparations
   Net imports of raw materials and semifabricated products

II. Combination of moyen and goods

a) Turnover in tangibles and prices
   Retail prices (clothing)
   Turnover in tangibles (clothing)

b) Long-term credit and workforce employment
   Long-term credits
   Workforce employment (full-time)

III. Money side

a) Movements of Accounts
   Credit transfers
   Postal cheques
   Notes in circulation

b) Movement of Credit
   Drafts issued
   Credits from the issuing bank

c) Money rates and interest rates
   One-month money
   Real values
   Cyclic movement
   Receipts from mortgage commitments

d) The three markets
   Commodities market
   Shares market
   Money market

e) Commodity prices
   Prices of tangible commodities
   Cost of living index (excluding accommodation)
   Wholesale prices index

2. Period II: the agnostic breakaway

2.1 The inauguration and consolidation of an empirical tradition

This second period opens with the empirical bias of the work of the National Bureau of Economic Research (NBER) in America, which relates to the classical definition of a business cycle proposed by Arthur Burns and Wesley Mitchell[10] in their master work of 1946, *Measuring Business Cycles*: "Business cycles are a type of fluctuation found in the aggregate economic activity of nations that organise their work mainly in business enterprises: a cycle consists of expansions occurring at about the same time in many economic activities, followed by similarly general recessions, contractions and revivals which merge into the expansion phase of the next cycle; this sequence of changes is recurrent but not periodic; in duration business cycles vary from more than one year to ten or twelve years, they are not divisible into shorter cycles of similar character with amplitudes approximating their own". This definition, morphological in nature, has remained an obligatory reference even in the most recent literature on cycle econometrics. In effect, it combines the qualities of clarity and flexibility. It is not restricted solely to a macroeconomic point of view since it emphasises the sectorial plurality of the cycle's manifestations, expressed as the conjoint movement of a large number of series: the cycle is a phenomenon which spreads throughout the economy. It proposes a simple and clear division of the cycle into phases and associates a terminology with that division. The succession of these phases defines a periodicity whose reference dates correspond to the turning points of the cycle: the passage from expansion to recession (*peak*), and the passage from contraction to recovery (*trough*). Finally, Burns and Mitchell regard the cycle more as a statistical than as a mechanical phenomenon: over the course of history successive cycles constitute a population that can be studied using statistical criteria, which can reveal any regular features that characterise them.

Without seeming to, the above definition involves important choices. It remains silent about the question of trend. It does not make the problematic identification of the trend of a temporal series a prerequisite for recognition of the cycles affecting it: each new cycle develops on the basis of structural features specific to itself, which determine the average level of activity over the cycle as a whole. To make two separate cycles in the same country comparable, it suffices to extract the difference between their average levels of activity. Finally, the definition does not aim to go into the distinction between several types of trade cycles, whether minor (often assimilated to the three-year cycles noted by Kitchin[11]) or major (the Juglar cycles lasting around ten years). It deals with a unique, non-decomposable cycle, and this allows it to remain flexible about the duration that can be envisaged.

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[10] The career of Mitchell (1874-1948) symbolises the succession of the first two periods. His work of 1913 (*Business Cycles*, University of California Press) combines theoretical and empirical considerations. It is a remarkable explanation of the sequential development of a typical cycle. His work of 1927 (*Business Cycles: The Problem and its Setting*, NBER), displays a certain distancing from theory without abandoning it. But the finding is that: "Between these two groups of workers, the theorists and the statisticians, there has been less communion than their mutual interests require" (p. 189). There is, however, a major continuity with the work of 1913: the emphasis placed on the role of profits in the cycle ("Profits as the Clue to Business Fluctuations", p. 105). Later, Mitchell was to devote himself to the empirical treatment of cycles, within the NBER framework.

[11] It was Schumpeter (1939) who gave the names Kondratieff, Juglar and Kitchin to the three categories of cycles, long, medium and short, that he supposed to fit into one another. It was in an issue of the *Review of Economic Statistics* of 1923 that the British statistician Kitchin and his colleague Crum each published an article pointing to cycles of the order of three to four years in the British and American series of prices and interest rates between the last decades of the 19th century up to 1914. Later, authors who returned to the question such as Abramovitz, would attribute the responsibility for Kitchin cycles to variations of circulating stocks, while Juglar cycles are more attributable to fluctuations of fixed capital investment.

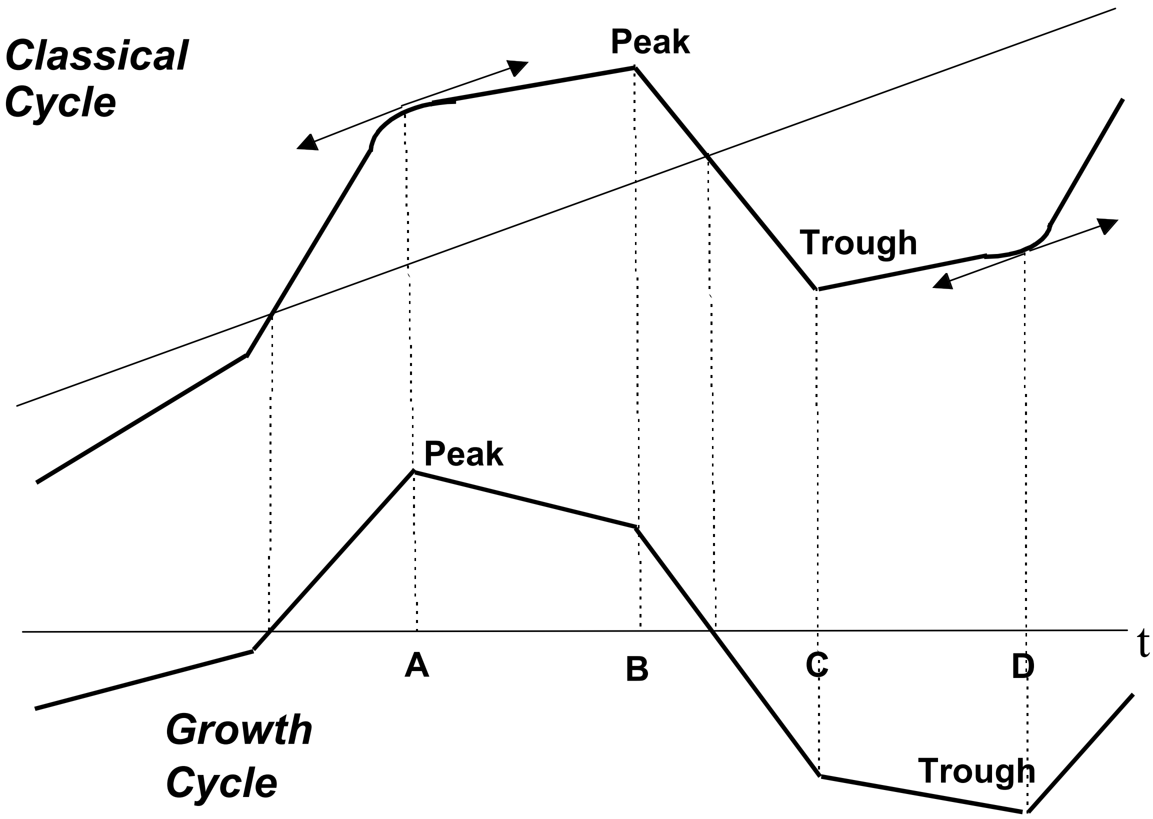
The fundamental intention of Burns and Mitchell is to treat economic cycles as a reproducible phenomenon, typical of competing market economies and able to be summarised in terms of stylised facts. That aim is preserved by later, more detailed technical considerations which re-examined some of their assumptions. In the post-war period the vigour of growth trends, the apparent attenuation and the greater asymmetry of economic cycles – with expansion phases longer and more pronounced than recession phases – led to a renewed tendency, in breaking down the series studied, to distinguish between trend and cycle: significant slowdowns of economic activity do not always result in its absolute reduction; it is difficult to note and quantify these slowdowns without identifying the long-term trend of that activity, below which the effective growth rate can fall without becoming negative. Henceforth the term used is *growth cycles*, as opposed to *classical cycles*, which were the first incarnation of *business cycles*[12]. The two points of view are not exclusive and can be combined, but the privilege accorded to one of them affects the reading of economic cycles, in particular their dating (Frame 2). In the United States the official chronology of these cycles has kept to the classical cycle point of view until now. Unfortunately, this choice of a point of view, which can and must be done clearly, is complicated by a blurring of terminology that depends on local custom and also on journalistic language, and which is hardly ever faithful to the standardisation proposed by Burns and Mitchell. This flexibility has at least one advantage, that of allowing business analysts to avoid endless repetition when they comment on the movements of trade!

Frame 2: Classical Cycles and growth cycles

Diagram 1 depicts a duality in the division of a cycle, according to whether the turning points are regarded as being the dates associated with the local peak and trough levels of the curve representing economic activity (points B and C on the time axis) – this being the image of the classical cycle – or the points corresponding to the maximum deviation from the long-term trend of economic activity (points A and D) – this being the image of the growth cycle. In the second case, therefore, the turning points correspond to times when the growth rate of activity is momentarily identical to the trend rate, after having exceeded it for a time and before falling below it in the case of a peak, and after having been below it and before exceeding it again in the case of a trough. When fully understood, this duality contributes to a detailed description of the cycle: the historical examination of cycles, especially in the case of the United States, often reveals a "blockage" phase that corresponds to the interval AB, in which activity has slowed down markedly without having already turned down towards its low point. Division in accordance with growth cycles re-establishes a certain symmetry of the cycle when the trend of activity is towards growth: the length of the descending phase BD increases relative to the ascending phase, in contrast to the classical cycle image in which it is reduced to the interval BC.

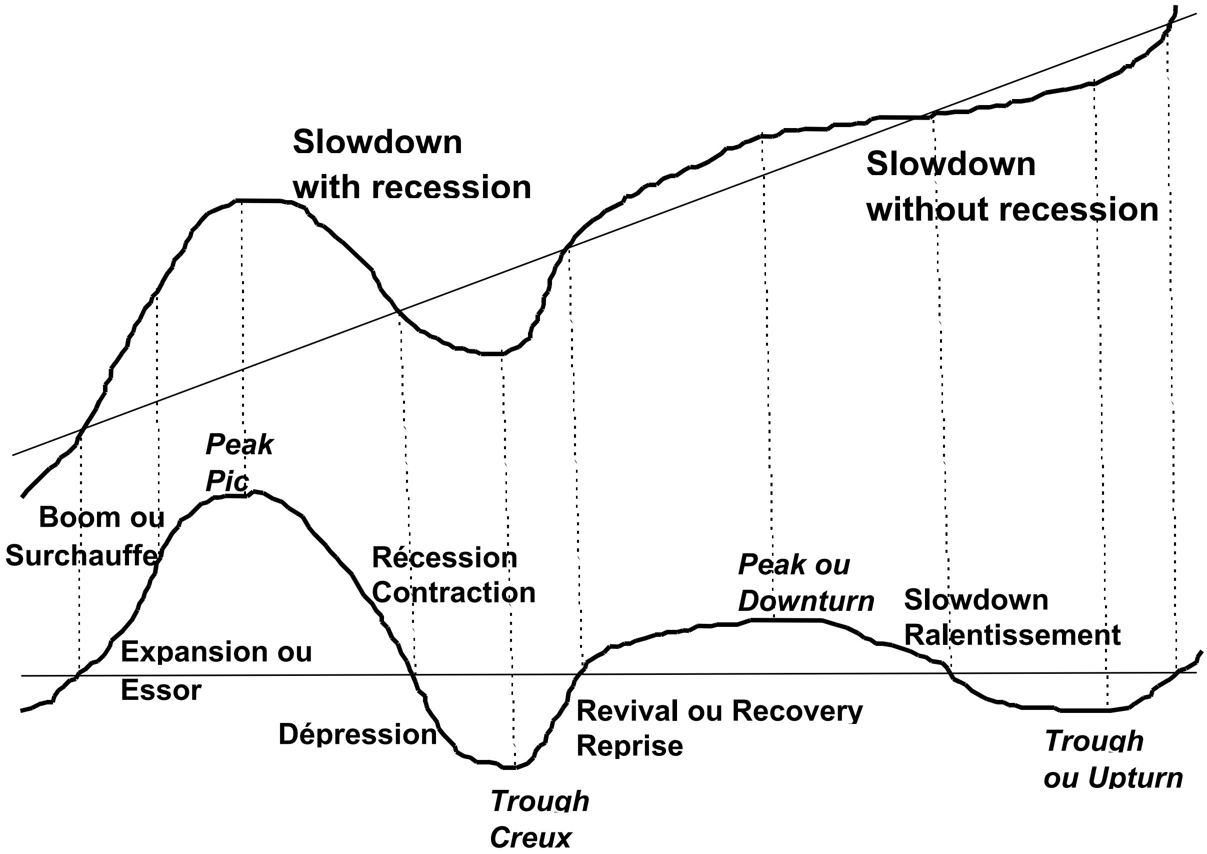
12 The study considered to have inaugurated the *growth cycle* approach, was published by Ilse Mintz in 1969 at NBER and dealt with the dating of *business cycles* in Germany during the 1950s and 1960s.

Diagram 1: The dual location of turning points



In the case of an economy that has been growing for a long period, some significant trade fluctuations have amplitudes too small to be recognised as classical cycles: they are not reflected by an absolute decline in activity. In the upper curve of Fig. 2, after a well-marked classical cycle, such a fluctuation appears: it takes the form only of what Anglo-Saxons call a "slowdown without recession", which does not interrupt the newly begun expansion phase of the classical cycle. On the other hand, the growth cycle image takes the form of a succession of two distinct cycles, since the "slowdown without recession" is pronounced enough for the effective growth rate to move below the trend rate for a time.

Diagram 2: Duality of cycle division and terminological uncertainty



This duality of the cycle division combines with terminological uncertainty to produce troublesome vagueness unless the conventions adopted are clearly expressed. Diagram 2 indicates a number of terms that are frequently used in practice: the term *recession* tends to be reserved for phases in which there is an absolute decline in activity, otherwise one speaks of a simple *slowdown*, knowing, however, that a recession often starts with such a slowdown; the term 'contraction' has lost its precision in relation to the terminology proposed by Burns and Mitchell, for whom a contraction represented a deepening of recession; it is in fact sometimes used as the equivalent of recession, and one speaks rather of a depression or *slump* to designate the aggravation phase of a recession, manifested by a particularly pronounced decline in activity.

This terminological uncertainty inspired a note of British humour in the journal *The Economist*: "But when does a recession become a depression? A cynical answer is: when your neighbour loses his job it's a slowdown (or, if you dislike him, a correction); when you lose yours, it's a recession; when an economic journalist loses his, that's a depression" (*The Economist*, "Depressing jargon", 23-29 January 1999).

The pride of place accorded to growth cycles seems appropriate for economies characterised by a monotonic (and most often positive!) growth orientation over a period of several decades. It is also helpful for the purposes of international comparison. In effect, the heterogeneity of national growth trends affects the comparability of national cycles, even in cases when they are closely associated: a decline of activity in a mature economy will have as its counterpart a slowdown in an economy that is catching up, so only the few very large-amplitude movements will seem common to a large number of countries when the classical cycle standpoint is favoured. Bodies whose vocation is international comparison and which engage in a systematic survey of business all naturally favour the growth cycle point of view: in the post-war period that has been the approach of the *Center for International Business Cycle Research* at Columbia University under the direction of Geoffrey Moore, and of the OECD, which is a federation of the large, developed capitalist countries.

The statistical separability between trends and cycles does not imply an obligatory absence of reciprocal causality between the two components. It simply means that macroeconomic regulation has a structural stability that is strong and durable enough for the reappearance of analogous causes during the long-term growth to result in a recurrence of typical sequences of short-term events in business. Certainly, the separation between trend and cycle is technically delicate to achieve: these two components of economic movement cannot be observed directly and must be extracted from overall observation of that movement by appropriate methods. The difficulty, intuitively, can be stated simply: the more the breakdown applied smooths the trend, the more it amplifies the cycle, and vice-versa. This difficulty is at the bottom of the methodological debates that continue to this day and that recur whenever new methods appear. The post-war technical innovations relate to the implementation of empirical methods for the iterative estimation of the trend of series subjected to examination, and logical rules for identifying the cycle's turning points. The notion of turning points, which introduce caesuras and rhythms into the pattern of trade over time, remains a central one in the growth cycle approach. The finesse of the task of identifying them, however, motivates a distinction (as made, for example, by the OECD) between major and minor business cycles, which modifies the idea of the strict unity of the cycle initially defended by Burns and Mitchell.

Application of these modernised methods allows the production of a large group of stylised actions. That group covers identification of the reference cycles of the GIP or the industrial production of various countries and zones, characterisation of the degree of similarity and synchronisation of these national or zonal cycles, and the observation of cyclic indicators in the regular behaviour that can play the part, in relation to these reference cycles, of advance, coincident or retarded indicators. This empirical synthesis is far removed from contemporary attempts at theoretical synthesis and still attracts the initial reproach formulated by Koopmans (1947) which has now become classical, that it is "measurement without theory": it relies on the number and extent of observations rather than on the testing of theoretical specifications by statistical inference. Thus, the initial effort of the NBER, driven by Burns and Mitchell, launched a programme of evolutive research that still persists today and continues to inspire and mobilise numerous practitioners of business studies all over the world.

2.2 Theoretical activism

The major crisis of the 1930s stimulated the inventiveness of many economists, who while paying due regard to the empirical dimension, concentrated their efforts on the construction of a theoretical point of view capable of taking into account the causalities that engender cyclic recurrence, but also the appearance of serious depressions within that recurrence, which deprive it of any automatism. This theoretical effervescence is pluralist and does not yield a unitary synthesis[13]. It does not lead to a generally accepted explicative model of the cycle that legitimises the empirical and agnostic position – but for all that, it is a finding that remains largely true up to the present, as recently stressed by Diebold and Rudebusch (1999). Three economists hold pride of place here, because the originality of their respective approaches has left lasting traces: Frisch took cycle analysis to greater depth; Haberler attempted an explicative synthesis; Schumpeter blew a breath of history into the theoretical approach.

[13] In a text that puts the history of the macroeconomic study of fluctuations into perspective, Olivier Blanchard (2000) has this to say about that period: "Business Cycle theory was not a theory at all, but rather a collection of explanations, each with its own rich dynamics".

2.2.1 Frisch: analysis

Ragnar Frisch, who began his work on cycles by modifying the methods of distinguishing between trends and cycles tried in the United States, was mainly known before the war for his writings of 1933-34 which regarded the recurrence of cycles as the product of a sequence of impulses and a propagation process. His thinking owed much to the Russian statistician Slutsky who had shown, at the end of the 1920s, that fluctuations which appear regularly in a quasi-periodic way could be brought about by an accumulation over time of erratic influences random in character. The impulses or shocks ("*the exterior impulse*" or "*erratic shocks*") that give rise to an economic cycle have a stochastic character and are therefore rather irregular; the propagation mechanism ("*the intrinsic structure of the swinging system*") transforms the sequence of such impulses into a succession of cycles whose length depends on the parameters of that mechanism, but whose amplitude is influenced by the strength of the impulses; this mechanism is essentially based on the principle of the accelerator, i.e. on the reaction of the level of orders and the production of equipment goods to the variation in the production of consumer goods. This basic principle is enriched by taking careful account of the production times of capital goods and of the liquidity constraints that affect consumers. The cyclic dynamic that results from these interactions may be complex and can span across cycles of different periodicity ("*long and short business cycles*"), which are not synchronised.

Frisch's point of view combines the exogenous and random character of the shocks with the determinist character of the propagation process. It is, besides, a point of view which was also adopted at the same time by Irving Fisher, who localised shocks mainly on the money supply side: according to him, in the long term their variations only have a nominal impact, but over a transitory period they give rise to price fluctuations which themselves produce product and employment fluctuations. Frisch's approach has undergone numerous developments until now. Whereas cyclic turning points fairly spontaneously suggest the idea of non-linear swings, for example from a period of expansion to a recessive period, Frisch showed that an account of aggregated fluctuations can be given in terms of a fairly simple representation of the economy in the form of a linear system affected by shocks. He emphasises the logical rigour that must inform the mathematical specification of the model representing this system.

2.2.2 Haberler: synthesis

While Frisch gives pride of place to analytical effort, it is to an attempt at synthesis that Haberler devotes himself in his reference review of theoretical explanations of cycles in *Prospérité et Dépression* (1937, 1943). Haberler takes each particular cycle theory seriously, but argues against it without definitively adopting the adverse thesis. On arriving at the conclusion of that exploration without prejudices, he embarks upon a patient patchwork process intended to draw up a synthetic table of the possible causes of cycles, beginning by freely borrowing partial elements from more unilateral theories. Into the impulse of the expansion phase Haberler brings the combined effect of the income multiplier and the investment accelerator, which gains from the good performance of profitability. The rate of expansion depends on the configuration of the financial circuits and behaviour. On the lending market, the demand for funds that results from intended productive and financial investments meets the supply, which sums provisions for depreciation, net savings, credit, and also the mobilisation of idle cash reserves. The interest rate depends on a comparison of the supply and demand curves for funds, but their position is influenced by income level: the Keynesian lesson has been learned.

For Haberler the explanation of a turning point into recession is not monistic. It calls into play the profitability difficulties caused by increased costs at a time of high stress on the supply of production factors, and the surprise these difficulties can constitute for entrepreneurs. It also involves the stresses on bank liquidity, connected with the degradation of relations between creditors and debtors. At the peak of expansion these stresses induce a specific dependence of the interest rate in relation to the supply of money. Yet, a downturn into recession is not obligatory: it can be postponed by a capitalistic intensification of production processes that reduces the pressure of wage and salary costs and provides outlets via investment.

The contraction phase shows phenomena symmetrical to those of expansion. The recessive effect of the multiplier and the accelerator does not have predefined limits, since the sensitivity of investment to pessimism about expected demand may persist even when the need for capital renewal seems to make renewed investment necessary. Contraction may go as far as a net de-investment. The priority of securing liquidity leads to a slide into deflation in which real and nominal magnitudes contract together. The deflationistic process wrecks buying power: money is desired as a reserve of value and a means to pay off debts, and no longer as the wherewithal to purchase goods. Relations between creditors and debtors undergo a general reorganisation involving liquidations and transfers of assets. This is a necessary stage for re-establishing the solvency of the surviving debtors.

The possibility of recovery rests on the restoration of profitability, favoured by the relaxation of production factor markets and by the movement to restructure and concentrate capital. Haberler focuses his attention on a precise point: to what extent is the pronounced and durable lowering of nominal wages and salaries a factor that promotes recovery, or on the contrary, one that defers it? He does not exclude an aggravation of the contraction due to the lowering of wages and salaries, when the saving achieved on them in a given branch also results in a diversion of purchasing power away from effective demand, in order for example to repay debts. Nevertheless, Haberler considers that eventually, sooner or later, the sustained lowering of wages and salaries will result in the creation of conditions for recovery by restoring the general conditions of profitability and liquidity throughout the economy: capitalists and bankers will not support recovery unless their enterprises regain accounts and balances that are judged adequate. This painful adjustment can be eased by an expansive public deficit policy.

Haberler's attempt at synthesis can clearly be contested when it favours one theoretical argument rather than another, but its rigour and ambition remain remarkable. It envisages an integration of multiple and partial causes within a single "epitome" of a cycle, a "virtual cycle", whose timing and morphology are a function of the productive and financial structures that are prevalent in the economy considered. Integration between real and financial variables, approached head-on, is central in the explanation of cycles.

2.2.3 Schumpeter: the breath of history

Joseph Schumpeter devoted prolonged thought to the cyclic form of capitalist development, from his early work *Théorie du développement économique* (1912) (Theory of economic development) until the historical and theoretical summation constituted by his work of 1939 on *Business cycles*. While being attentive to statistical methods and observations, Schumpeter is nevertheless sceptical about the methodology developed by Mitchell (with whom he corresponded): the statistical dating of cycles and turning points is too sensitive to exogenous shocks, to irregularities of all kinds, that affect the endogenous development of the cyclic movement without having other than a contingent relationship with it. It is the logic of that development which interests Schumpeter, even though its direct observability is problematic. The cycle is a phenomenon of deviation from and return to an equilibrium situation and this theoretical point of view, deliberately absent from the work of the NBER, determines the pertinent historical reading of statistical series: if one were able to spot real situations close to equilibrium over the course of time, one would have a more correct picture of the trend of economic development. Since this is difficult, one can at least do one's best to achieve a "symptomatic" reading of temporal series, aiming to discern the signs of disequilibrium.

Equilibrium is a situation recognised as normal by economic agencies because it conforms to their expectations. They expect it to persist and incorporate that expectation in their normal behaviour. It is not necessarily a static condition, but may be one of relatively steady growth characterised by stable parameters, such as the savings rate, and one whose inflections are slow. Deviation from equilibrium is of microeconomic origin and is brought about by the efforts of entrepreneurs to innovate and by the provision of credit for that purpose by bankers. This is the product of a two-way bet on the future, that of the entrepreneur-innovator on his creativity, and that of the banker on the capacity of innovation to create a pure profit that will enable his loan to be repaid. The entrepreneur-innovator and his banker constitute the driving partnership of capitalist dynamics. Implementation of innovations mobilises resources and generates a wave of expansion, during which the resources available are redeployed in favour of entrepreneur-innovators who have access to credit. When the innovations reach maturity, the arrival of new products on the market modifies the normal equilibrium of activities and prices, and the return to equilibrium – a modified equilibrium – adopts the path of recession because previous and competing activities are eliminated. The most elementary outline of a cycle corresponds to this succession of prosperity, which engenders a move away from equilibrium, and recession, which leads back to it. But during this sequence, driven by innovative creativity, it is the nature of the equilibrium itself that has evolved: the trend of economic development is not external to the cyclic movement, but is its product.

This elementary outline quickly becomes more complex because the primary expansion directly associated with innovations is amplified by a secondary wave, which consists in carry-over effects upon existing activities. When credit is mobilised to finance not only innovations that bring productivity gains but also the induced development of existing activities, expansion is amplified still more and can degenerate into a speculative boom, at the risk of a more serious subsequent fall: the transformation from recession to depression can be brought about by the need to eliminate overextension of production capacities and to correct speculative drifts. A four-phase cycle not unlike the NBER outline is obtained: the return to equilibrium by recession is unstable, and the depression that follows brings activity to below the equilibrium level. Once the imbalances have been eliminated, recovery finally allows renewed expansion in the direction of the new equilibrium. The complete cycle corresponds to the sequence of these four phases - prosperity, recession, depression and recovery - around the path of equilibrium which plays the part of attractor. Schumpeter goes further. Taking into account the plurality and heterogeneity of the flow of innovations, he injects the elementary outline of the cycle into a multicyclic concept of economic development which embodies, in an entirely geometric harmony, short (Kitchin type) cycles, medium (Juglar type) cycles and long (Kondratieff type) cycles.

The systematic nature of Schumpeter's overall construct raises doubts or rejection (besides, he himself warns against a dogmatic reading); this, however, should not cause one to underrate the pertinence of his analytical intuition and the importance of the question he hands down to practitioners of business analysis. For Schumpeter the long-term tendency of an economy, its *trend*, has no intrinsic and pre-existing reality. It is the product of a development that hardly takes the form of a continuous and steady growth, but rather, that of an evolution whose rhythms are imposed by cycles of various periodicities. These cycles are brought about by the very driving forces of the said evolution – the flow of innovations and the credit which finances them – and their interaction progressively delineates the trend that emerges from them. An ideal forecasting approach should make explicit *ex ante* the effect of routine and innovative behaviour that engender the trend before it congeals into past history. In practice such an approach is impossible, because it comes up against the unforeseeability associated with the very nature of innovations. Prediction relies more modestly on the more or less extrapolative, more or less reasoned extension of the trend hitherto, which can always be plotted *ex post*. Schumpeter's evolutionary concept, which established his understanding of economic history as a process of mutation and selection, did not seem to him incompatible with the identification of trends by statistical methods, provided one is not deceived by their forecasting deficiencies.

3. Period III: professionalisation under the Keynesian influence

During the first post-war decades business analysis was consolidated as a specific professional practice conducted in institutions with general interests (such as the Institut National de la Statistique et des Etudes Economiques [National Institute of Statistics and Economic Studies], or INSEE, in France) or in specialised bodies. It fitted into national contexts that show appreciable differences: the Anglo-Saxon tradition of empirical *business cycle* analysis developed as far as the creation, maintenance and use of a group of advance, coincident and retarded cyclic indicators. Spreading to international bodies such as the OECD, for a long time that tradition only had limited practical reverberations in France, while the term *conjoncture*, derived from Latin, which emphasises the originality of the group of economic circumstances specific to a given period, is used operatively only in the countries of continental Europe. Practitioners of business analysis developed a normative and didactic body of principles intended to guide the description, explanation and forecasting of trade movements (in French economic administration this was the successive work of Alfred Sauvy, André Vincent and Jacques Méraud). They adopted a technical armoury that became more and more diversified (from surveys of business opinion in companies and households, to three-monthly national accounts) and also more and more universal: instrumental unification contributed towards the attenuation of national peculiarities. They fitted their approach into a concern to assist macroeconomic decision-making, intended to counterbalance the planistic tone of the period.

The NBER's agnosticism certainly took root. The pragmatism of business analysts and their attention to current events are a two-sided coin: they give protection against a theoretical speculation that is often prodigious in subtle models, but not very conclusive; this can degenerate into a journalistic glibness, which uses repetitive common ground to comment on the economic situation and which is too close to information that may be revised. Yet, new links were formed with economic theorists under the aegis of a culture inspired by Keynes, which pervaded the quite new instruments of business analysts. Surveys of business opinion are nothing else than an instrument for measuring "gut reactions", the state of confidence and motivation to invest. The framework of national accounting, which adapts itself to the short-term timescale with three-monthly accounts, facilitates the adoption by analysts of a macroeconomic understanding with neo-Keynesian dominant features: the reference to basic quantitative adjustment mechanisms, such as the duet comprising the income multiplier and the investment accelerator, popularised by Samuelson (1939), imposes itself naturally. The construction and use of macro-econometric models designed for forecasting, which materialise macroeconomic knowledge in the form of a vast, coherent group of accounting and causal equations subjected to empirical validation, was the crowning achievement of that process. They constitute the tools for explaining and forecasting the complete range of global interdependences between the agencies and variables of a national economy. The Keynesian inspiration was wedded to the modelling project instituted in particular, from the decade 1930-40, by the Dutch analyst Tinbergen (1937).

3.1 The master's intuitions …

The Keynesian inspiration is not devoid of ambivalence. Sensitive to the depth of the deflationist depression of the 1930s, Keynes proposed a scheme for analysing sequences of economic events that lead to engulfment in such a situation and demonstrated the need for public action of a structural nature to emerge from it. It is this absence of distinction between the business situation and structure that largely constitutes the originality and interest of the *General Theory*. Doubtless also, its limits: Keynes was the economist of a moment in history, but a moment even so. Among other things, the *General Theory* is a partial theory of business.

The best of Keynes's heirs, the Harrod-Domar duet to begin with, would be among the first to call attention to this Keynesian incompleteness.

Keynes attributes a key role in the turning point that marks the end of the cyclic expansion, to the interaction between the incentive to invest and disappointments relating to the effective return from investments. These disappointments trigger the recession and induce agencies to favour the preference for liquidity. They open the way to a deflationist slide into an equilibrium of under-employment: the lack of confidence among bankers and entrepreneurs can inhibit recovery, even when monetary policy intends to encourage it by lowering the nominal interest rates. Businessmen, traumatised by disappointments related to demand and profits, feel uncertain about the future to such a point that they abandon investment projects whose profitability seems to them too uncertain. For Keynes, no principle is available for a spontaneous response to this weakness in a capitalist market economy. Cyclic chains of events are not automatic: their occurrence depends on the configuration of expectations and the quality of their co-ordination, via the institutions. As a function of these conditions, certain supposedly transitory moments of the cycle may degenerate into stable states: recession becomes an equilibrium of under-employment. This absence of automatism in the sequence of cycle phases is a question that has been approached more technically by econometrists who are interested in the phenomenon of "*duration dependence*": to what extent does a phase of expansion or depression have greater chances of coming to an end the longer it persists? According to a recent review by Diebold and Rudebusch (1999), phases of expansion, but not contraction, died more easily as they aged before the first World War, while rather the reverse has been the case since the second World War, at least in the United States.

Keynes discusses in particular the role of falling nominal wages and salaries in the course of depression. Certainly, capitalists benefit in the short term from that decline. But if it leads to expectations of cumulative decline, the economy will slide into deflation and under-employment since it is always in the interest of entrepreneurs to wait for new wage and salary reductions before committing themselves to investment and job creation. What is important, according to Keynes's reasoning, is that the fall of nominal wages and salaries has a negative effect on the overall economy, not only nor even mainly because of its immediate impact on consumption, but because it results in a lasting disincentive for capitalists to invest. It undermines the viability of capital accumulation, both by increasing the cost of debts inherited from the past (Keynes revives the argument in this connection developed by Irving Fisher in an article in 1933), and by increasing uncertainty about the future. It is therefore creditors and people of independent means who become dominant against producers and wage-earners. The former can adapt to low growth, or even deflation, if this results in an excess of savings that favours the acquisition of securities rather than the development of activities.

Keynes delivered a lesson in method, which is a precious asset. The representation of equilibrium and short-term dynamics depends on assumptions about the rigidity of prices and the formation of expectations that determine the nature of the interactions between the markets for commodities, securities and labour. The reasoning must be adapted to the originality of each business situation, to the modes of adjusting imbalances, and to the nature of the expectations that characterise it. Emergence from depression does not happen in just one way. The Keynesian approach underscores the plurality and contingency of the conditions that govern the evolution of the economy. Hicks (1939) further formalised the Keynesian intuition via the concept of temporary equilibrium, which indicates how the current equilibrium of an economy depends both on the legacy of past history and on expectations about the future.

3.2 … and the doubts of his heirs

To create a more complete and accurate image of short-term business movements within economic history, however, the Keynesian contribution should be brought up to date. Since the post-war period, that was the purpose of the series of articles by Harrod and Domar, who developed an open and always stimulating line of thinking on the intrinsic instability of capitalist economies. Whereas an investment level high enough to mobilise the savings available is a condition necessary for full employment, maintaining it dynamically entails making explicit the ways in which the new supply contributed by investments is to be absorbed. The realisation of those investments must create enough income for the new production capacities not to supplant one another, for lack of sufficient outlets. One can therefore point to a "justified" or "*warranted*" growth rate that balances the conjoint effect of the income multiplier and the investment accelerator: that equilibrium rate gives rise to an incentive to invest that matches the new supply and demand generated by the new investment. If established, companies are induced to persist in the course of their spending at the same rate: growth is self-sustaining. But close to that rate, instability prevails: deviations from the equilibrium rate, even if small, originate cumulative movements of expansion or depression. This instability is connected with the predominance of quantitative adjustments influenced by expectations that render the incentive to invest volatile. It is also influenced by the asymmetry between the factors of labour and capital, between the endogenous character of the path of balanced capital accumulation, which depends in part on the subjectivity of enterprises, and the inertia of demographic processes, which does not allow spontaneous coincidence between that path and the growth of full employment (see Frame 3).

Harrod and Domar were aware of the incompleteness of their thinking. Concerning the growth model he was presenting; Harrod (1948) expressed himself thus: "Although these equations clearly demonstrate the instability of an expanding economy, they do not on their own constitute very good tools for analysing the course of depression". It was an appeal for deeper reflection on the short-term instability of capitalist economies. The nature of that deeper reflection could lead to other theoretical readings of this instability, in particular the classico-Marxist affiliation, whether they emphasise the under-consumerism that threatens capitalism or the menaces that affect the profitability of capital accumulation. Domar (1947) expressed capitalist instability in a very suggestive way: "This is remarkable characteristic of a capitalist economy: while in general unemployment is a function of the difference between real income and production capacity, most of the measures (i.e. investment) undertaken to increase the national income at the same time increase production capacity. It is probable that the rise in national income will be greater than the production capacity increase, but the entire problem lies in the fact that the rise in income is temporary and is gradually resorbed (by the multiplier effect), while the capacity has been increased once and for all. Accordingly, in relation to unemployment, investment is at one and the same time a remedy against the disease and the cause of greater problems in the future".

The theoreticians of the glorious 1930s devoted a substantial part of their efforts towards making explicit the factors that can embody or give rise to capitalist instability. The aim became to set up theories of growth that demonstrate the capacity of modern capitalist economies to keep, apart from short-term fluctuations, to a path of balanced and steady growth, close to full employment. Along such a path, those economies overcome the impact of technical progress on production and the distribution of income; investment, employment and consumption develop at compatible and harmonious rates, accompanied by sizeable and steady increases in the productivity of labour and by stability of the efficacy of equipment capital. Solow emphasises the importance of the plasticity of the production function and of the combination between capital and labour, as a function of their relative cost, in warranting the path of balanced growth. Kaldor recognises the local instability of capitalist economies but attributes a key role to adaptation of income distribution in ensuring overall stability:

this is a reassuring view of the cycle, which normally participates in regulating the growth of modern capitalist economies. The theoreticians of the Fordist persuasion, who demonstrate the historical emergence and institutional consolidation of a Taylorian productivism that creates salarial incomes, unite the Marxist and Keynesian inspirations. The dynamism of capital is tamed to contribute to the quantitative and qualitative improvement of the salaried condition, to the point where one can speak of a "salaried society" to describe post-war societies. This taming depends on institutional mediations that orientate the action of profitability criteria. Those mediations take place mainly in Nation-States, which provides a strong basis for interventions of economic policy, both to cope with economic shocks and to give impulse to the collective conditions for growth.

The realities of the 1950s to 1970s nourished these attempts at "synthesis": business fluctuations became subordinate to a powerful and virtually steady growth dynamic. Until the beginning of the 1970s the options of economic policy were discussed as a function of their efficacy in relation to maintaining that growth: monetarists advocated rigorous control of the money supply to avoid sliding into inflation; neo-Keynesians laid most stress upon budgetary policy and action on income distribution to warrant the path of balanced growth.

This positive and normative thinking, which standardised the disturbing Keynesian contribution and reduced the inquiries of Harrod and Domar to those of apprentices with little experience of balanced growth, revealed their transitory character with the difficulties of the 1970s. The time to re-open the file on instability had returned.

Frame 3 : An analytical presentation of the Harrod-Domar model

The presentations of the so-termed Harrod-Domar model, which are frequent and numerous, are not always very satisfactory, no doubt because the model does not exist as such and reflects the personal synthesis of their ideas proposed by one reader or another of Harrod and Domar. Here, particular reference will be made, with a little further development, to the reading proposed by Hahn and Matthews in a survey of the theories of economic growth, which is already old but remains a reference text in the field[14]. In effect, it seems appropriate for highlighting the advantages still possessed by the Harrod-Domar model for business analysis.

1 - The existence and nature of balanced growth

It is first necessary to characterise the nature of a path of balanced growth at a constant rate indicated by the model for an economy that produced goods by means of technology having complementary factors (capital et labour) and that saves a constant fraction *s* de son income *Y*. Endowed with the hypothesis of a stock of productive capital appropriate to the level of production, entrepreneurs strive to maintain that proportion. The investments they wish to make I^d are proportional to the expected variation of the product:

$$I^d = v\Delta Y^a \qquad (1)$$

where *v* is the coefficient of capital under the normal conditions of technology implementation.
At equilibrium, les expectations and the intentions of the agencies are realised. The realisation of investment expenditure amounting to I^d brings into play the income multiplier to determine the effective product *Y*:

$$Y = \frac{I^d}{s} \qquad (2)$$

For equilibrium to be established completely, the effective variation of the product ΔY must confirm that which had been anticipated:

$$\Delta Y = \Delta Y^a \qquad (3)$$

Dividing by *Y* and using the expressions for *Y* et for I^d taken from (1) and (2), one obtains the "guaranteed" (warranted) growth rate of the economy:

$$g_w = \frac{\Delta Y}{Y} = \frac{s}{v} \qquad (4)$$

The interpretation of this rate should not be misunderstood. It is the rate that would prevail if all the conditions for the economy to remain on its equilibrium path at all times were satisfied. The positive dependence of that rate in relation to the saving effort does not mean that increased savings would spontaneously lead to more growth, starting from any arbitrary initial situation. It means that if the rate of saving is high relatively to the unitary needs for capital *v* , the equilibrium growth rate must be large enough for the investment induced to absorb the savings available, and growth will therefore take place at a constant rate. This is indeed a Keynesian reading of balanced growth at a constant rate.

[14] This survey was originally published in 1964 by The *Economic Journal* under the title "The theory of economic growth: a survey". The French version, supplemented by an appendix, to which reference is made, was published in 1972 by Editions Economica under the title *Théorie de la croissance économique*.

2 - Instability around the path of balanced growth

The balanced growth rate $g_w = \frac{s}{v}$ is virtual : what chance does it have of becoming actual if the starting point is a situation of imbalance? Will imbalances correct themselves, or propagate? Analytically, one must pass on from the recognition of a solution of balanced growth, described in section 1 above, to an exploration of the dynamics of imbalance. Hahn and Matthews did this by adopting a formalisation in continuous time, certain implications of which are developed here. A variable with a dot over it designates its derivative with respect to time (two dots indicate its second derivative): $\dot{x}_t = \frac{dx_t}{dt}$. In what follows, investment corresponds to the derivative $\dot{K}_t$ of the stock of capital K_t with respect to time. We begin from an initial situation in which the effective coefficient of capital $V_t = \frac{K_t}{Y_t}$ differs from the normal or desired coefficient v that is involved in the balanced growth rate.

The multiplier of income and activity is assumed to act instantly, such that at any moment equality of savings and investment is achieved for a product level characterised by:

$$Y_t = \frac{\dot{K}_t}{s} \quad (5)$$

The effective g_t growth rate $\frac{\ddot{K}_t}{\dot{K}_t}$ of the $\dot{K}_t$ investment is thus identical to that of the product Y_t which is designated by :

$$\frac{\dot{Y}_t}{Y_t} = \frac{\ddot{K}_t}{\dot{K}_t} = g_t \quad (6)$$

Hahn and Matthews then introduce a discretionary hypothesis according to which entrepreneurs decide upon their investments by striving to adapt their capital stock instantly to the anticipated growth g_t^a and taking into account the difference between the desired capital coefficient v and the effective coefficient V_t. This is a deliberately harsh version of the accelerator :

$$\frac{\ddot{K}_t}{\dot{K}_t} = \frac{v}{V_t} g_t^a \quad (7)$$

By combining (6) and (7), we obtain :

$$g_t = \frac{v}{V_t} g_t^a \quad (8)$$

Accordingly, after a few elementary transformations, the difference between the effective growth rate and the warranted rate can be written:

$$g_t - g_w = g_w \left(\frac{v - V_t}{V_t} \right) + \frac{v}{V_t} \left(g_t^a - g_w \right) \quad (9)$$

Hahn and Matthews make the assumption that the difference $g_t^a - g_w$ has the same sign as the difference. $v - V_t$ The ideal is this: if producers are, for example, short of capital ($v > V_t$) because they have been taken unawares by the recent amplitude of growth, they modify their expectations by anticipating a growth g^a above the trend of growth g_w . In equation 9 the two terms of the right-hand part then act in the same direction and lead to an effective growth rate higher than the warranted rate. The first term represents the impact of the effort to adapt the capital stock, the second term the impact of growth expectations. A positive surprise concerning growth leads to growth more rapid than the balanced growth that would exist if producers had revised their expectations by an appropriate amount.

Moreover, this deviation from balanced growth is explosive: equation (8) shows clearly that the correction of expectations is not sufficient to eliminate surprise relative to the effective growth, because an upward revision of expectations itself feeds a still stronger effective growth due to additional needs for capital. It is the difficulty in making good this lack of capital that is the source of this instability. In effect, in such a figurative case how does the effective capital coefficient V_t vary?

By definition:

$$\dot{V}_t = \frac{\dot{K}_t}{Y_t} - g_t V_t \qquad (10)$$

which, taking (5) and (8) into account, becomes:

$$\dot{V}_t = s - v g_t^a = v\left(\frac{s}{v} - g_t^a\right) = v\left(g_w - g_t^a\right) \qquad (11)$$

Retaining the same assumptions about behaviour, the lack of capital ($v > V_t$) leads to an expected growth higher that the balanced growth and to a ***decrease*** of the effective capital coefficient V_t . The lack of capital does not correct itself but is aggravated, because the combination of optimistic expectations and the suddenness of the behaviour of the accelerator gives rise to product growth so rapid that capital cannot adapt itself to the level required. The impact on the deviation from balanced growth is summarised by a modified form of equation (9) , which uses the relation (11) :

$$g_t - g_w = g_w\left(\frac{v - V_t}{V_t}\right) - \frac{\dot{V}_t}{V_t} \qquad (12)$$

If the effective capital coefficient falls when it is below the desired coefficient, an initial positive deviation between the effective growth and the balanced growth will become progressively larger.

To render the model specific, one can consider the example of a simple scheme for the formulation of expectations in accordance with which producers extrapolate the growth they have just observed:

$$g_t^a = g_{t-d} \quad (13)$$

where d is the time taken for the producers to adapt their expectations to the growth perceived. Equations 8 and 11 become :

$$g_t = \frac{v}{V_t} g_{t-d} \quad (8')$$

$$\dot{V}_t = v\left(g_w - g_{t-d}\right) \quad (11')$$

Starting from a situation in which, at the same time, the recent growth rate extrapolated by the producers g_{t-d} is higher than the warranted rate g^w and the capital coefficient V_t is below its desired level v , this dual imbalance will spread and become aggravated.

The assumptions that govern the instability of balanced growth in the Harrod-Domar model are clearly specific. It would be unreasonable to wish to draw from them a general representation of business fluctuations such as the conditions that govern long-term growth. The model ignores the restoring forces that can operate over various horizons. But it enables the instability to be underscored, which may give rise to a business cycle if economic activity goes through a phase that combines a marked reactivity of investments with the perception, by enterprises, of capital insufficiency. This is explained by the lag of expectations, even subject to revisions, compared with the effective acceleration of growth. These are modes of adjustment which amplify an initial deviation from balanced growth.

3 - The interference of instability with natural growth

Finally, there is a last point dealt with by Harrod and Domar: what relationship is there between effective growth, warranted growth and natural growth, which in the absence of technical advances corresponds to the growth rate of the labour supply g_n , of the demographic order? In the long term the question seems to be very simple: equality of the warranted growth rate g_w and the natural growth rate g_n is not guaranteed unless an explicit mechanism of demographic or economic adjustment is specified (Solow deals with this by introducing the interchangeability of capital and labour). Underemployment may increase in the long term, or else growth may be rationed by a shortage of labour. This long-term problem, namely that of possible chronic unemployment or rationing of growth, is a priori quite distinct from the vagaries of business cycles.
In fact, the long-term relation does not prevail independently of shorter-term interactions which Harrod refers to allusively: demographic growth influences the effective demand, which depends on the needs of a larger number of individuals, and therefore the growth effectively recorded. The effective growth rate g is not independent of the natural rate g_n.
A natural rate higher than the warranted rate does not imperturbably result in increasing chronic unemployment, but can bring about an increase of the effective rate and so give rise to a wave of expansion, by virtue of the very economic instability described earlier. Conversely, low demographic dynamism is not automatically favourable to full employment if it contributes towards suppressing effective growth. But this is a subject no more than touched upon in the articles of Harrod and Domar, namely that of the interaction between demographic growth and business dynamics.

4. Period IV: The cycle between determinism and uncertainty

During the last quarter of a century it has been economic reality itself that has shaken the habits adopted by business analysts. Suppressed during the glorious 1930s, uncertainty has returned and is scattering the hopes placed in an instrumental and technocratic command of the rhythms of economic life. Business analysts are testing the limits of analytical and forecasting tools adapted to the reasonable and contained instability of the post-war growth. Not only is instability becoming more dangerous, but it seems to be gaining in spreading force within an international space which is both globalised and heterogeneous. This evolution leads to the search for business analysis methods that could become the *common asset* of business analysts, whose exchanges, if only driven by necessity, are more regular and more frequent on the international scale.

It is in such ground that a revival of the analysis of economic cycles has taken root. New generations of theoretical models and econometric methods have been proposed. The history of tools and that of theories are once again closely linked, as at the beginning of the century. It is a mobile situation, rich in unconcluded debates in which competing visions of business fluctuations and, more generally, different ways of "recounting" economic history are expressed. It is not easy to distinguish between the stimulating use of new techniques and the flowering of disputable theoretical reflections. A given technique may be introduced into highly differentiated "experimental protocols" and so may be mobilised by competing theoretical visions. It is also possible for methods to embody within them a very specific conception of the dynamic, which makes them resistant to certain theoretical options. Neutrality of techniques is not a foregone conclusion.

The theoreticians called "neo-classicists" pitilessly perceived the weaknesses of the large macroeconometric models produced by the synthesis of the 1960s between neo-classical and neo-Keynesian positions. The specification of those models was in most cases static and was adapted after the event to introduce in an *ad hoc* way the dynamic and uncertainty naturally incorporated in the temporal series used for their estimation. This dynamic essentially amounted to a linear determinism sprinkled with transitory and standardised random contingencies, and to expectations which were largely a function of previous changes. In contrast to this reductionism, the critics of the standard modelling proposed and tried out techniques focused from the start on the dynamic of the temporal series studied, its sensitivity to the nature of expectations and its intrinsically random character. These techniques aimed to explore the structure of temporal causalities without *a priori* restrictions. They also aimed to respect the stochastic nature of the economic dynamic, which pits the rationality of the expectations formulated by the agencies against the irreducible uncertainty of the shocks that affect the latter. In effect, they competed against a renewal of the statistical approach to temporal series under the influence of other scientific disciplines confronted by problems of analogous nature, such as physics.

The properties of the methods proposed are frequently of an asymptotic order, i.e. in order to be validated they require a large number of observations. They are therefore methods which lend themselves in particular to the study of a prolonged period with short periodicities (yearly, three-monthly, monthly, etc.). Their aim is to specify and assess processes that unite the need for long-term equilibrium relations within a group of variables and flexibility in the short-term adjustment modalities between those variables. Thus, they correspond to the lines of theoretical thinking that are concerned to define rigorously a long-term equilibrium path that can constitute a reference for appreciating the shorter movements of economic history.

4.1 Disparate conceptions of the cycle

It is not an incontestable and unified paradigm that has imposed itself during the past twenty years or so, but rather, a shift in the positioning and description of problems that continue to raise divergent responses. This is what applies to the factors and nature of the cycles that affect contemporary economies.

The models of "*real business cycles*" perceive fluctuations as the result of optimum adaptation of a competitive system comprising rational agencies, to unexpected shocks. In these models, the inter-temporal rationality of the agencies, via substitution and income effects, transforms a sequence of independent and unforeseeable shocks (or innovations) into fluctuations that show signs of self-correlation, in other words, apparent cycles. For example, if an unexpected shock brings about an increase of interest rates, immediate work becomes more advantageous than future work because savings are better remunerated: this results in fluctuations of the offer of employment. One can also consider the illustrative example of a win on the lottery, unconnected with productive effort: even if the player is highly rational, equipped with calculations of the probabilities, he will still be surprised if he wins the jackpot. Depending on the nature of his preferences at the time, his propensity to spend at once or to put his winnings into savings, or perhaps to work less, his rational decisions will induce rapid or slow inflections of various economic variables. Applying these ideas, models of real cycles have to be subjected to a process of calibration, i.e. justified choice of the numerical values of their parameters, some of which are difficult to estimate by classical econometric methods.

However, as pointed out by one of the founders of this approach, Plosser (1989), the word 'cycle' is hardly appropriate here and it would be better to speak of fluctuations when referring to transitory components of activity that have no confirmed recurrent character. The innovations that give rise to these transitory fluctuations have a permanent impact that affects the long-term course of events. Thus, growth and "cycles" are fundamentally inseparable since it is the same chronicle of innovations that engenders them. It is the degree of persistence of these shocks and the way their impact is propagated that must be represented as well as possible, by identifying "response functions" of the economy to the shocks, which are necessarily of diverse origin, both monetary and real. The empirical validation of models of real cycles is based on the capacity of these response functions to simulate appropriately the effective dynamic of the economy, which can be summarised by a few key indicators (such as the temporal variances and correlations of the variables observed).

In the course of a polemic debate with Plosser, Mankiw (1989) pointed out that this representation was only compatible with the stylised empirical facts provided that it exhibited innovations of a nature and intensity such as to be able significantly to modify the behaviour expected from a competitive system. For example, in a recession that depresses employment and so increases the marginal productivity of labour, real wages should increase compared with a competitive labour market. If this is not what happens, it is because the recession is correlated with a negative technological shock that affects the function of production and reduces the marginal productivity of the factors: real wages then revert to a procyclic evolution which accords better with the stylised facts. When technological shocks are measured by the variations of the productivity of the production factors in the manner of Solow's residue, their macroeconomic order of magnitude is sufficient to provide naturally the chronicle of innovations needed for the theorisation of cycles for that reason called real. However, Mankiw was indicating that he had doubts about interpreting the chronicle of technological shocks so measured as a sequence of exogenous and independent innovations. On the one hand, they in fact appear to depend on the course of the activity itself, via the well known phenomena of production factor adjustment lag, employment in particular; on the other hand, when they are unusually intense this can manifest the impact of singular events, such as the oil shocks, which cannot be reduced to routine innovations. A large part of the variance of the apparent chronicle of technological innovations can in fact be explained either by a productivity cycle endogenous to the cycle of the activity, or by singular events. If, by eliminating these two components, it were possible to isolate the true innovations of technological order, it is not certain that they would still have sufficient statistical intensity to bring the model into line with the facts.

These criticisms formulated against the first generation of "real cycles", developed in the 1980s, led to a dual adaptation: on the one hand, acceptance of the diverse nature of the shocks that can affect the economy; on the other hand, a reconciliation with the "neo-Keynesian" stream which wished to remain faithful to a more traditional view of fluctuations and open to a rigorous representation of the market imperfections that give rise to them – such as price viscosity, monopolistic behaviours or even bankruptcies. Thus, making the most of the fruits of this reconciliation, Blanchard (2000) discounts a new synthesis centred on models of dynamic and stochastic general equilibrium, uniting greater freedom in the nature of shocks and expectations with the rehabilitation of the role of market imperfections in supporting the propagation of fluctuations – a customary theme in post-Keynesian literature.

The models of real cycles, or more generally from now on, the models of dynamic stochastic general equilibrium, constitute a theoretical experiment that explores in depth the hypothesis that macroeconomic fluctuations are produced by (more or less …) rational microeconomic behaviour, whose methods of empirical validation obey specific norms. Whatever judgements may be made in their favour, they remain very far from the preoccupations of practical and forecasting business analysis and cannot really be used for that purpose[15]. In that sense they are certainly no substitute for the macroeconomic models of neo-Keynesian design, the criticism of which nevertheless constituted one of the reasons for their development. They are models whose purpose is a searching analysis of a precise problem, rather than to understand and forecast economic series of events in which nothing happens "other things being equal".

4.2 The difficulty of methodological transparency

The debate raised by questioning the neo-Keynesian inspired macroeconomy, then by emulation of the "neo-classical" and "neo-Keynesian" streams, brings up important issues of methodology. This relates in particular to the agreement between theoretical model and empirical method, an old methodological problem made more acute by the greater depth of techniques, which does not always go along with the necessary transparency. To describe the temporal processes observed in a suitable way, the methods used must be adapted to the nature of these processes. What the processes are, however, is not a matter of absolute ontology but arises from the choice of a model to represent them. By choosing a model, the proper method is prejudged. And behind the recourse to a particular method, there is a reference model. But how can one be sure about the indisputable singularity of such a model? The tests one can envisage enable the reference model to be accepted or rejected, but it is much more difficult to choose between the population of models that can be considered. Deduction and induction compete for the legitimacy of the analytical techniques used[16]. And how does one escape from this epistemological vicious circle, other than by the discretionary choice of a "model-method package" that meets the preferences of the economists concerned?

15 For a precise demonstration of this point, see Bonnet and Duchêne (1998). It is also a point made by Diebold and Rudebusch (1999, p. 21): "Still, even if the new structural models are off to a good start, they nevertheless have a long way to go if they are to be truly useful for macroeconomic forecasting".

16 On the need in this context for active methodological reflection, reference may be made to the thoughts expressed by Malinvaud (1991).

It would also be best for these preferences and the coherence of the "package" to be stated clearly. Thus, the dynamic of a "real cycle" can formally be written in terms of deviations from the equilibrium path of steady growth associated with the neo-classical model retained. To compare the "real cycle" of theory against suitably described stylised facts, it must be possible to identify that path by an appropriate statistical trend extraction method. Things are rather less than clear in this domain, because it is quite difficult to prove that such an extraction method does the job of revealing the equilibrium path associated with a precise model. And everybody prefers their own, without too must justification.

Accordingly, there is always tension between a wish for greater technical rigour by recourse to analytical methods that take better account of real dynamics, and the use of these methods as weapons in the service of adopted theoretical positions that do not always avoid a speculative drift. That tension plays a part in the current incompleteness of this *fourth period*. The situation is clearly as stimulating as it is unsatisfactory. Dialogue between those who adhere to different approaches is needed to overcome it, the more so since there is still an association between the technique used and the type of job they do: practical business analysts who use the toolbox of descriptive statistics on one side, and theoreticians of cycles and dynamic econometrics of temporal series on the other.

The entire course of the past history described brings out the permanence, in successive forms, of the tension between the deterministic and stochastic approaches to economics. Thus, the procedure adopted by Burns and Mitchell represented a relaxation of the hard determinism implicitly present in the Harvard barometer by a study of the statistical frequencies that characterise the various aspects of the cyclic phenomenon. Determinism is nevertheless a natural propensity of business analysts. When one makes a forecast, the care devoted to identifying the linkages that can be envisaged is a response to the need to be persuaded of their high degree of probability or their necessity. It is mentally difficult to argue that a forecast is well founded without being convinced of the reality of a quasi-necessary sequence of causes and effects. *Ex-post*, the importance attributed to the account by business analysts in their analyses corresponds to the need to propose a rational and convincing reconstitution of linkages which were in fact very difficult to foresee: even taken in reverse, determinism always tends to have the last word. Although quantitative forecasts are useful, this is *inter alia* because their errors motivate this work of reconstitution and orientate it. It is a little like detective novels: the mystery is solved when scattered clues converge to bring out the evidence of a sequence of events that is difficult to identify and foresee in real time.

The raw material that business analysts work with consists in effect of perceptions, expectations and reviewable intentions which are those of the economic agencies themselves. The measurable macroeconomic reality is the product of the conflicting aggregate of the decisions ultimately made by those agencies. It is a random and non-linear reality where, for example, adjustments at different times can happen suddenly and reverse the economic trends rapidly, so taking by surprise even those observers who are best warned of their probable proximity: economic reversals rarely have the gentle and rounded shape claimed for them by elementary models of business fluctuations. The macroeconomic trade situation is not only vulnerable to unpredictable shocks whose origin is external to the economy considered, which upset the plans of the agencies and their realisation, but rather, uncertainty is fundamentally endogenous in it. It is a function of the interactions that unite the agencies within an economic system characterised by a monetary and financial organisation, a regime of competition and accumulation, a group of institutional rules which model the typical regulation of that system. The practical study of the economy thus moves away from a clear cut opposition between macroeconomic holism and methodological individualism, because it compels an effort to understand the interaction between macroeconomic regulation and the plans of the agencies, and therefore constructs its own observation tools, such as surveys of business opinion.

The notion of a cycle so brought into play is far from a conception of economic life that would reduce it to the rhythms of a clockwork mechanism, however refined. This danger exists when, in the press but also in some professional cases of economic thinking, cycles have sometimes been regarded as a reality completely exogenous to the strategies of the agents and economic policy in particular. On the contrary, recourse to the notion of a cycle should here be seen as the heuristic way to take account of the interaction between the force of certain economic determinisms and the influence of stochastic factors, whether they arise within the very functioning of market economies or stem from the impact of specific shocks, including decisions of economic policy.

5. Outline of a contemporary view of the cycle problem

Following in the footsteps of Burns and Mitchell, as a first approximation a cycle can be regarded as a succession of fluctuations of the macroeconomic aggregates that has characteristics of duration, amplitude and profile sufficiently similar for that succession to be considered as a recurrence. The property of cyclicity that characterises the course of these aggregates can be understood as the significance and regularity of the recurrence. The cycle is regular when the instability factors that displace the economy from a balanced path do not exceed a certain threshold, their action being progressively contained by other factors that limit the deviation and reverse the dynamics. If the cycle were sufficiently regular, economic forecasting would be easier and so would learning about cycles. Such learning, by both the social agencies and economists, would decrease business uncertainty to its irreducible fraction summarised by the distribution of probabilities relating to the various "business conditions that can be envisaged": for economic agencies capable of progressively building up a subjective estimation of that distribution, the management of cycles would be a particular dimension of risk management. And without doubt, in the world of industry, entrepreneurs have long since acquired experience of the management of cyclic risk, which they know can threaten the balancing of their books.

Although economic history has known periods in which this cyclic regularity seemed to impose itself and become an object of familiarity to economic agencies, it also shows that this can rarely be taken for granted. The cycle is a phenomenon located at the intersection of the factors of stability and instability that compete within economic life. That competition renders the fate of cycles much more uncertain than an inescapable return to the starting point. The virtual development of a business cycle is by its nature subject to tensions and shocks that affect its regularity. Since cyclic development includes in particular the moment of crisis and the gravity of the crisis can vary, the behaviour adopted by the agencies when faced by it influences subsequent events. The institutions and the rationales for action they embody are then subject to conflict, the more so since the immediate perception of the crisis complicates the definition of policies simultaneously suited to the different short and long term horizons. It is often during the critical moments of a cycle, when financial crises threaten or a deflationist trend prevails, that, with some difficulty, institutional changes take shape, notably in the monetary and financial domain, which will then mould subsequent developments.

This duality of the notion of cycles can be defined by locating the use of that concept at the intersection of approaches inspired by regulationism and evolutionism. These two methods of approach seem in effect rather to complement one another in reaching an understanding of how a cycle is the complex form assumed by the trial and error attempts that govern a society's learning process of its long-term economic course:

- In a regulationist approach, the periodisation of economic history is based on the observation of successive regimes of accumulation and growth. When the structural stability of a regime is sufficiently confirmed, the participation of the cycle in its regulation bears witness to the formation but also the correction of imbalances. An empirical distinction between trends and cycles is then relatively easy, without signifying any strict independence of these two manifestations of the dynamic. Over a fairly long period the dynamic is characterised by recurrences based on typical sequences of behaviours, tensions and corrections, so that the decomposition between trend and cycle has a descriptive and analytical range. A trend is like a sequence of states that the modes of regulation and the judgement criteria of the agencies lead one to regard as normal. Cyclic regularity expresses the capacity of the economic system to correct its imbalances and revert to a near-normal condition. The reproduction of cycles is vigorous enough to contribute to the characterisation of the accumulation regime and to participate in the maintenance of the trend: cycles are both reproducible and reproducers. When the combination between trends and cycles that prevailed up to a point becomes obsolete, that may be a symptom of the emergence of structural instability which calls the regulatory mode into question.

- In an evolutionist approach, the learning and adaptation processes of the economic agencies, which orientate the selection of their behaviour and their strategic choices, are the source of the economic dynamic. Macroeconomic regularities emerge and are then consolidated by interactions between agencies which, frequently, combine within critical situations distanced from equilibrium (for example, when engulfment in cumulative situations of under-employment and deflation motivates new social compromises). The long-term course, marked by its initial conditions and the effects of inertia, is also subject to bifurcations in the direction of new economic regimes. Recurrences are part and parcel of that course, but they do not signify a systematic return to equilibrium. Cycles can express the reality of reverses and even failures in the collective effort to found a new structural regime of growth. These relapses, manifested by recessions and depressions after the precarious trial of new organisation methods during the expansion phase, mask conflicts between social agencies in defining the necessary mutations. They bear witness to the difficulty of reaching stabilising social compromises that relate to the objectives (the "collective preferences") and the processes that allow them to be achieved.

To be inspired by regulationist and evolutionist references at the same time is to take the ambivalence of the cyclic phenomenon into account. At times that phenomenon shows a regularity favourable to the control of risks and to the reproduction of growth performances consistent with the trend or, on the contrary, fails to ensure the success of changes and manifests a painful and unexpected reversibility contrary to the viability of long-term plans. A rather reassuring reality in the first case, in the second case a cycle is worrying because of its possible destructive effects, when its depressive phase shows up an institutional anomaly that is hard to overcome.

The structural stability of a growth regime and the regulatory nature of the cycles that drive it are not independent of an accord between the expectations of the agencies and the fundamental properties of that regime. For example, the solvency of debtors is not independent of a consensus with creditors about the growth perspectives. Where there is no such accord, the surprises and disappointments induced by the contrast between expectations and reality, the revisions to which their formation is consequently subjected and the conjoint adaptation of behaviours give rise to fluctuations specific to these periods of transition. During them, the economic agencies learn new norms that guide the implementation of their rationale. So long as this maturation is insufficient for new models of expectation coherent with the structurally modified determinants of the growth regime to impose themselves, the fluctuations remain disordered, more perturbing than regulatory. Return to a normal reproduction of a redefined growth regime is in part connected with a renewal of the dominant model of inter-temporal rationality.

The process described leads to a use of the notion of cycles which is both intensive and prudent. Cycle analysis in a powerful instrument for working on economic chronology, one which allows the observation and identification of characteristic chains of events, the location of an economy's position in the course of a dynamic, and the relocation of the current economic situation in the series of past experiences. Since the complete course of the cycle involves time delays that exceed the horizon of strict short-term forecasts, it displays a temporality that lies at the intersection of economic and structural movements. Thus, mobilisation of the notion of the cycle is part of a conception of business analysis which makes of it a tool for bringing structural issues to light. This conception rejects a pure and hard dualism of business and structure, which would underestimate the structural density of business movements and the fact that historical bifurcations can be determined within precise economic situations. It motivates the inclusion of economic reasoning in the historical approach.

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CYCLES AND BAROMETERS: HISTORICAL INSIGHTS INTO THE RELATIONSHIP BETWEEN AN OBJECT AND ITS MEASUREMENT

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"Examine the causes and indicate the effects of the business crises that have taken place in Europe and North America during the 19th century". This question, proposed by the Academy of Moral and Political Sciences as a competition topic in 1862, led to the publication by Clément Juglar of his treatise on *business crises and their periodic recurrenc*e, which was awarded a prize by the Academy. The work is marked by two innovations. The first is the abandonment of the old notion of a crisis and its replacement by that of a cycle. Instead of describing the succession of events associated with each economic crisis in a specific way, Juglar and his contemporaries in the second half of the 19th century sought to substitute an appreciation of their similarities and differences in a spirit of comparison, locating each crisis in a paradigmatic series and reducing the crisis to no more than an element and moment in a more general and more abstract object, namely the cycle. Since then, *Business cycles* (BC) have very much become an object of economic science. From the 1880s to the 1940s they even became the topic of predilection for economists. Juglar's second innovation was to answer the Academy's question not at the usual level of *a priori* reasoning applied to economic policy, as had been done by Malthus and Sismondi, but at the level of empirical evidence: from a compilation of publications by the American Congress or the Bank of France he traced a detailed history of business movements, country by country and period by period. In this he was one of the initiators of a descriptive approach in terms of what would much later become known as stylised facts. And besides, to the question of causes posed by the Academy, he responded with a work "less ambitious but more satisfying" – as the rapporteur commented – which identified several factors, the most important of them related to the deregulation of credit, while the others were totally outside the financial sphere but played the part of symptoms perfectly well. The good Dr Juglar put in place the setting for barometers long before they came into existence: the nosology of crises interested him far less than clinical expertise.

The same will apply to our historical work. Although the history of economic facts and the history of economic ideas form the essential back-cloth to what we wish to say, what this paper is about is neither the cycle nor the concept of cycles, but the relationship of both with the statistical tools of a dual representation of facts and concepts. Among these tools and integrating almost all of them, economic barometers were the preferred and magical instrument used by the first business analysts at the beginning of the 20th century. Built up from the elements of economic statistics available at the time – serialisation of data, graphical representation, transformations, decomposition, correlation, mathematical modelling – they were designed to give an account of the cyclic fluctuations of economic activity and to test the theories put forward to explain such fluctuations.

"To give an account of" is an expression that can have several meanings. First, it means *to represent*. Constructing and publishing an economic barometer first entails the objective recognition and depiction of an oscillatory movement of the economy as a whole in a numerical and graphical line that can be manipulated in the form of a small number of combined statistical series. The mere quantitative representation of a cycle already poses serious problems: is there really such a thing as a "cycle" which pre-exists its own measurement, as might be hoped in the context of a realistic epistemology? And if so, how is that cycle to be defined, conceptualised and distinguished from any other form of evolution of economic activity? How can one be sure that this indeed is what one is measuring? Then, is the measurement being made on the correct assumptions, on the correct scales? What are its formal properties, its semantic properties?

But to *represent* is also to give a presence, to bestow a reality. Would a barometer rather not be a construct which, by the very choice of what it treats as important, institutes business cycles by giving them a concrete form which is a working definition? In this "operationalist" vision in the sense given to it by Bridgman in 1927, "it is the measurement itself that defines what is to be measured, which does not pre-exist its own measurement", to quote Jean Ullmo (1969, p. 24) precisely. Methodological questions about measurement lead fairly rapidly to the epistemological question of the type of theoretical knowledge procured by the empirical approach: how can one know anything (in economics) about the properties of an unobservable theoretical object (the cycle) from the empirical measurement of phenomena selected for all sorts of good and bad reasons?

It seemed more interesting to us to answer all these questions from a historical point of view, bringing in the agencies and issues of the period, than from a purely philosophical point of view or in terms of an epistemology with no historical or social context. This must lead to a better understanding of "science in the making".

Another reason in favour of the historical approach is that a barometer is not a thermometer. It is not limited to recording a trace. It reveals phenomena, suggests causes or reasons, heralds a probable future, guides a possible response, an organised action which may change the course of events. Beyond its representational function, a barometer plays a part in the action, which can besides take three fairly similar but distinct forms: a heuristic role of assisting an understanding of the economic mechanisms involved, a predictive role of forecasting crises, and a decisional role of help to economic policy makers. If it is added that economic barometers were developed in two different contexts, that of advice for private enterprise and that of national business analysis institutes whose function is to support public decision-making, it is evident that these instruments have played an important part in the birth of business analysis *expertise*. By following the history of the barometer we will be able to determine the scope of its significances and usages, which covers the very wide spectrum of knowledge and action, and enables consideration of the relations between an economic situation, its trace, the manipulation of that trace, and the action in response to the economic situation.

1. Conditions in which barometers emerged

The golden age of economic barometers was the inter-war years. The first barometers, however, appeared towards the end of the 1880s. Why not earlier? Certainly, because the recurrence of industrial and business crises reached its peak at the end of the 19th century and that decade was marked by a first Great Depression. The crash of the Banque de Lyon et de la Loire, and then that of Union Générale in France in 1882, was the first of such magnitude and brought about a fall of the currency that was not reversed until 1896, the beginning of the "*Belle Epoque*". At that time the crisis also affected Great Britain and marked the beginning of its decline. In France the parliamentarian Méline pushed through a law which instituted a dual customs duty. But the whole of Europe and the United States soon slid into protectionism after decades of liberalism. The Welfare State made its appearance to correct the disastrous effects of the second industrialisation, more particularly in France in 1891, with the creation of the *Office du Travail* (Office of Employment) which a few years later was to absorb the *Statistique Générale de la France* (SGF) (General Statistical Office of France). The career of French statisticians at the beginning of the new century, such as March, Huber, Risser, Dugé de Bernonville and Bunle, was marked by this new involvement of the state in matters related to employment and the economy: participation in the *Conseil Supérieur de la Statistique* (Supreme Statistics Council) (1885-1912), the Commission des crises (Crisis Commission) (1908), the *Comité d'études relatives à la prevision des chômages industriels* (Study committee on the prevention of industrial unemployment) (1911), and the organisation of surveys on prices, wages and the cost of living.

It was in effect for this reason of a political agenda as much as for reasons of innovation in statistical techniques, that a period of extraordinary development of economic statistics began: in 1885, at the very time when Marshall was giving his inaugural lecture in Cambridge and when American economists were founding the *American Economic Association* (AEA), the International Statistics Congress of Quetelet which had last met in 1872 arose from its own ashes in the more academic form of the *International Institute of Statistics*

(IIS). Francis A. Walker, first President of the AEA in 1885 was also President of the *American Statistical Association* (ASA), which had been created in 1839, from 1883 until 1897. However, the link between economics and statistics was not fortuitous but only indicates that economists were becoming more open to a statistical analysis that corresponded to social and governmental demand, an opening to which statisticians responded all the more willingly as their long and virtually exclusive association with demographers was coming to a close, and because the interest of public agencies in their discipline reinforced its establishment. The growth in the size of Statistics societies during that period was remarkable, both in the USA – the ASA grew from 160 members to over 500 between 1889 and 1898 – and in France for example, where the membership of the Société de Statistique de Paris (Paris Statistical Society) increased from 81 to 412 between 1881 and 1884. This revival of the learned societies went together with the appearance of new journals such as the ASA's *Publication* (1888) or the *Review of Political Economy* (1887) which saw itself as an alternative to the omnipresence of liberals in publishing. It also went together with the development of the first courses and chairs of mathematical statistics (Hotelling in the USA) or economics and statistics in England (*the London School of Economics*) and in France (Faure chair in Paris in 1892, doctorate and *agrégation* in economics created in 1895 and 1896), and correlatively, with a surge of publications of treatises on economic statistics in the two following decades (Bowley 1901, Liesse 1905, King 1912, Yule 1911, Secrist 1917).

A last factor worth mentioning is the taste of the period for mechanical inventions, for studies of movement and most particularly for the recording of all kinds of phenomena, as witnessed by the work of Marey (1885) on kinematics, and for photography. Here, one could revert to the theme of the "mechanisation of power" so dear to Giedion to show that the end of the century was marked in the arts and industries as in the sciences by the fertility of studies of movement. In movement there is something enigmatic which, while being material, lies beyond the visible and requires new technologies to record the permanent trace of its effulgence. From that trace it will then be possible to work back to the mechanism that leads to the movement. Marey's photographs of birds, Muybridge's horses, Marcel Duchamp's nude coming down a stairway, Taylor's observations on the handling of pig iron, Frank Gilbreth's wire constructions and his invention of "therbligs" as atoms of movement were aspects of the same search as that of economists who wished to scrutinise the movement of the economy using a special device which they called a barometer.

2. The first barometers

So what were the characteristics of the first barometers? The Austrian statistician von Neumann-Spallart used the quite new framework of the IIS to propose a kind of general picture of symptoms that would enable the position of a national economy to be fixed and an international comparison to be sketched out, which extended the unrealised project of the international congress of statistics in the 1870s to establish an international statistical system. That picture envisaged "a measure of the variations of the economic condition of nations" by virtue of three batteries of indexes. A first group of indexes gives an account of the state of the economy: production of oil, cast iron, zinc, glass, armaments; passenger traffic, goods, navigation and external trade. A second group of indexes reflects the social condition of the system: consumption of tobacco (in France) or beer (in Germany) or coffee (in Austria), deposits in savings banks, emigration, bankruptcies. Finally, the state of society's morale is appraised by a last series of indexes, which owe a lot to the pioneers of moral statistics led by Farr, Quetelet and Guerry in the 1830s: frequency of marriages, birth rate, ratio of illegitimate births, suicides, criminality. The IIS Bulletin reproduced Neumann-Spallart's report and tables of figures (1887) "unfortunately without including the graphs he had used to illustrate it", and the author's death that year precluded any further publication.

Fortunately, that same year the French Statistician de Foville, Head of the office of Statistics at the Ministry of Finance, put forward at the congress of the French Association for the Advancement of Science and then at the Paris Statistical Society an "*Essay on economic and social meteorology*" which essentially amounted to commentary on a strange device with coloured strips, which he presented as follows:

> *"You all know what recorders are. That is what we call mechanisms of greater or lesser ingenuity whose purpose is not solely to follow from day to day the variations of some phenomenon or other, but in addition, to take note of those variations, commit them to paper, in a word record them, either in the form of curves, or by chemical coloration, or in some other way. There are recording barometers, thermometers, hygrometers, photometers, anemometers, etc. Well, Sirs, I have tried to achieve something similar in the field of economics. By constructing this image I have tried to portray in a manner as real, simple and expressive as possible, the fluctuations of our country's economic activity over a certain number of years." (de Foville, 1888, p. 243)* ***[Fig. 1]***

The principle of this barometer, which is identical to that of Neumann-Spallart, consists in summarising economic and social activity by means of a small number of indexes – in this case 32 – which reflect and quantify the various dimensions of economic movement, which "bear witness" to them in their way according to the author's terminology, within a very heterogeneous not to say heteroclite panoply in which furniture sales are taken together with sales of postage stamps, pledges at pawnbrokers and the number of suicides. The originality of the device, however, lies in the graphical semantics that replace the cold language of figures; instead of giving a numerical series, each index is translated into a strip coloured "*with a red square for a good year, a pink one when the year is only fairly good, sombre grey when the year is mediocre, more bad than good, and finally, black to characterise really bad years*." This "childish" colour symbolism, in the author's own words, which replaces the figures of the indexes by a complex recoding which is evoked but not described[1], is the product of a claimed wish for the greatest possible clarity: each of the coloured strips reveals much better than would the figures, the succession of phases in the business cycles, the "fat kine and the lean kine" of the biblical parable. But it also enables a second reading of the diagram, vertically and in synchronism, which shows up the similarities between the coloured cycles: "*At every stage of the picture, the evolution is the same. In every case there is a little black on the left, red in the middle and a lot of black on the right. It is like a ray of sunshine between two unequally dark clouds*". In other words, the same movement of business straddles all the economic phenomena and is in some way the common factor between them, latent but shown up by the measurement and the coding, since all the indexes show the red and black trace nearly simultaneously.

Only nearly, however, because the agreement is not at all perfect. Some indexes precede the others in the same cyclic evolution. "*Among the various elements of our national economy there are some which are influenced more quickly and more strongly than others*." This off-set is neither fortuitous nor the result of measurement errors, it is the very principle that makes this instrument of synthesis a forecasting tool: de Foville was able to end his article with a reassuring look forward to a return of the fat kine.

The main ingredients of the barometer are there: juxtaposition of fairly heteroclite series, but ones chosen as a representative sample of economic activity transformed by a certain semantic, graphical and/or numerical rule, to become comparable and to bring out synthetically both a similar movement and useful differences.

The barometers of these two precursors attracted many followers in the first decade of the 20th century. But some, such as Clément Juglar or Baron Charles Mourre, favoured a *single index* that would serve to represent BCs: the first chose the portfolio of the Bank of France or even its liquidity, which moves in the inverse

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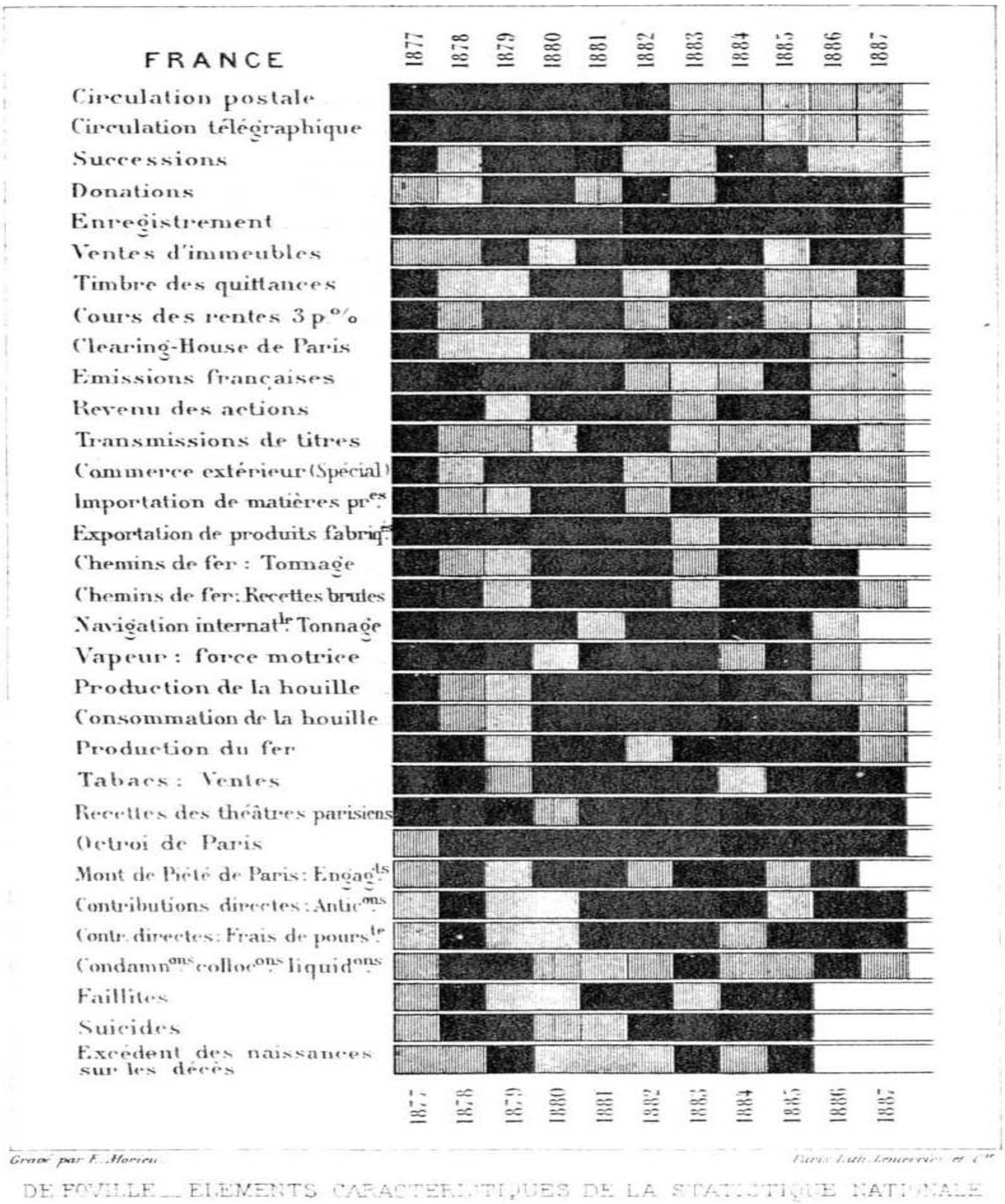
[1] Sometimes it is levels, sometimes growth rates that are taken into account; sometimes maxima and sometimes minima are interpreted in red or pink as favourable.

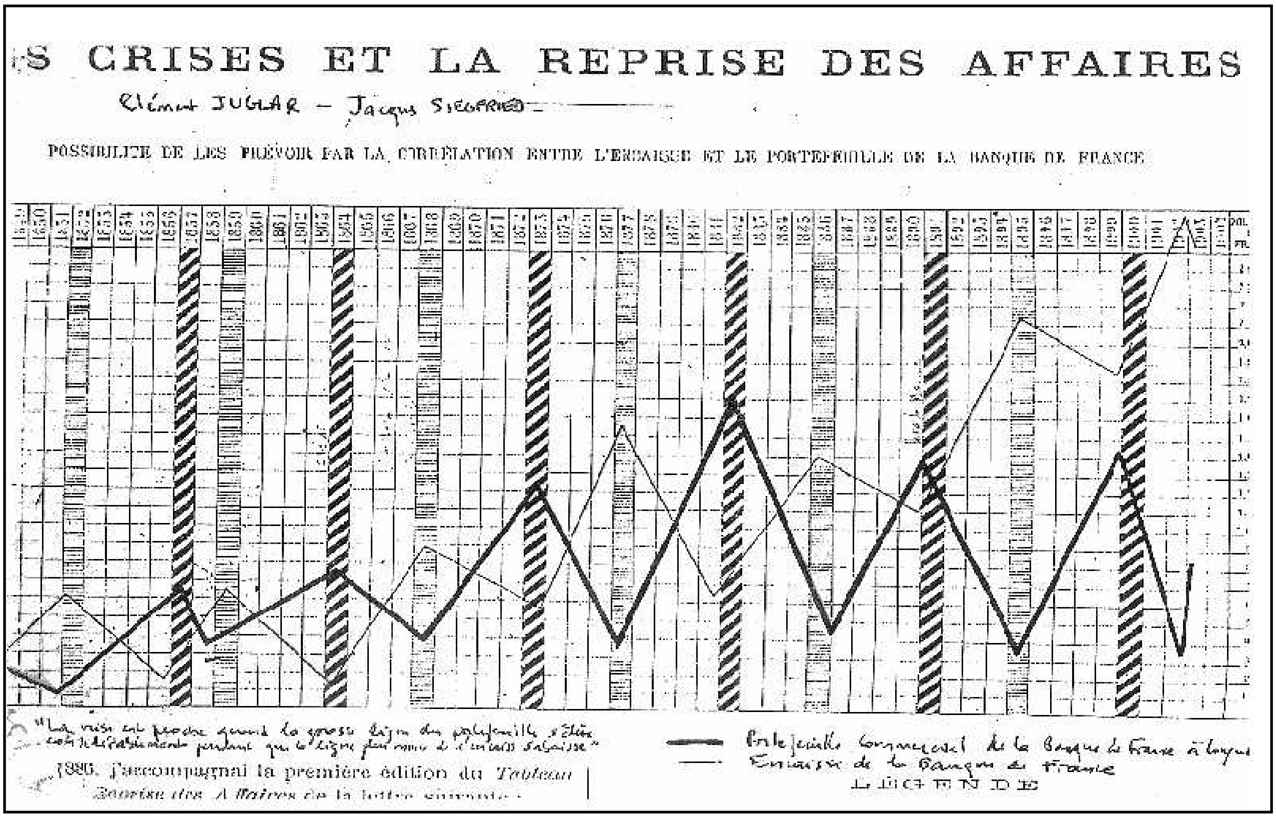
Figure 1: de Foville's barometer (1888)



direction **[Fig. 2]**, while the second advocated commercial paper. Some, more recently, have seen an excellent index of economic activity in the volumes of packaging cardboard used.

Other authors defend the *synthetic index* that results from an aggregation of several plain series: since 1922 the company ATT has published a single Index of General Business, and Carl Snyder, director of the Federal Reserve Bank, collected 56 series into 5 synthetic indexes (production, wholesale distribution, retail distribution, general business, finances). In 1923 Warren Persons published an Index of Trade for 1903-1925, which combined 28 series, and its most famous offspring, the Harvard barometer, is still of that type. The questions raised by synthetic indexes are similar to those known for price indexes: what justifies the sample of series chosen? Should they be homogeneous (because they measure a well defined dimensions of economic activity) or heterogeneous (the better to cover the diversity of that activity)? Under what conditions (for example, of reduction to comparable units) can the combination be reduced to a sum or an average? Does that average have a probabilistic interpretation which gives a better estimation of the true size of the phenomenon? Is it the proper way to neutralise accidental variations so as to isolate more effectively the constant cause of the phenomenon measured? If there is an average, what are the weightings of the various series?

Figure 2: Juglar's Barometer



The synthetic index reflects the entire series of questions raised by the theory of averages stemming from a transfer by Quetelet from the theory of errors in the social sciences and highly criticised at the turn of the century by the German (Lexis, Bortkiewicz) and English (Venn, Galton, Pearson) schools. At its 1923 meeting the IIS pronounced against the use of composite indexes: "*it is not expedient to group indexes that represent phenomena of different nature into a composite index*". This opinion was echoed by most authors on BCs before 1914.

This certainly motivated many statistical economists, such as Lucien March, Director of the SGF in France or Armand Julin in Belgium, to remain faithful to a system of numerous indexes (43 in the case of Julin, 1911) grouped into dimensions. For Julin, for example: production, trade, incomes and consumption, demography and morale. No system combining these indexes into a single index or a small number of them is to be found in their barometers. Beveridge in London in 1909, Mortara in Rome in 1913 and Sorer in Vienna in 1913 all developed barometers of the same type as March and Julin in the framework of public statistics, and this right up to the first world war.

3. Semiology

It is not just a question of the technical difficulty of aggregating heterogeneous series. It is also that the vision of those pioneers was that of semiology, a term borrowed from the field of medicine, a group that included a fair number of economists, Juglar to begin with. His work on business crises had already stressed "*what is known in medicine as a predisposition*", which determines the form of the crisis, or even the *upset and blocking of a paralysed* social *body*, the necessary *liquidation* or *purge* that follows the crisis, and the affirmation that "*crises, like illnesses, are inevitable*". The first economic barometers resembled temperature and blood-pressure charts of an economy subjected to periodic attacks of fever. Beveridge published his barometer with the title "Pulse of the Nation". For Schumpeter (1939) cycles, like heartbeats, were part of the essence of the organism in which they appeared. The term 'semiology' was also adopted in the small book by André Liesse, professor at the CNAM, on Statistics. *Its difficulties, procedures and results* (1905), four chapters of which were devoted to "the study of statistical symptoms or semiology", and by the decision of the IIS (1911) advocated by A. Julin to set up "*a special commission entrusted to study the methods relating to statistical semiology*". Wagemann[2] defined semiology or the science of symptoms as "*an inductive process leading to the discovery of the probabilities of events (…) which does not, however, yet amount to a theory of the necessary relationships (…) Besides, transition from the theory of symptoms to functional theory* [defined later as the reciprocal determination of economic variations which do not lend themselves to mathematical formulation] carries a certain risk. There is nothing less prudent than to assert the existence of functional or causal relationships before the results obtained from the theory of symptoms have shown them to exist". In this "medical" vision barometers are incapable of noting any mechanisms, identifying their causes and factors and their way of acting upon the system. The very functions of measuring the wellbeing of peoples and acting as a forecasting tool are rejected by statisticians such as Julin. This brings us back to the thermometer, which by objectively detecting a high temperature, offers the clinician no more than a symptom.

Another sign of this semiological approach is the use of demographic variables. In a causal economic process one would look either for natural causes, as for example Jevons did by referring to sun-spots, or for endogenous causes, as economists have preferred to by invoking the phenomena of anticipation (Pigou), innovation (Schumpeter), machine production time (Spiethof, Aftalion), cash flow fluctuation (Hobson, Marx) or money supply (Hansen, Fisher). But it could never be said that variations in the marriage or the crime rates caused economic cycles. It may be that they are the results of such cycles, but the reasoning that establishes this is not obvious, as we shall see. All that observation can do is to record "concomitances", to repeat a term that John Stuart Mill associated with one of the four canons of inductive method in his *Logic*. Recourse to such extra-economic variables – demographic or moral – is much more easily justified in the context of a semiological view in which high or low values of these variables constitute signs that are symptomatic of an economic condition because they appear in systematic association with that condition, without being either causes or factors of it.

In his work on Business Cycles, Schumpeter devoted a large part of his introduction to what he called "Common sense Semiology[3]" and defined as "the interpretation and co-ordination of such symptoms". He even gave a non-limiting list of 41 such symptoms, and above all provided an interesting justification for it: the "factors" of a business cycle should preferably be sought among those with causal significance, but this is almost impossible because we do not always have a rational scheme (in the sense of rational mechanics) and, all too frequently, "*in our field causes do not always precede effects (because) anticipation of coming events sometimes produces the same effect on the behaviour of business communities as do these events themselves, and symptoms sometimes lag behind the events to which they refer*." (Schumpeter 1939, p. 14 - 15).

The forecasting function, rejected by Julin, is in contrast at the centre of the "private" barometers produced by small consultancy offices in companies, one of the models of which could be the "*Babson chart*" accompanying the weekly Letter sent out by Roger W. Babson to his subscribers since the early 1910s. And to attach importance to forecasting is to favour a single, synthetic index. Constructed on the basis of 12 statistical series (4 commercial, 4 monetary and 4 financial), Babson's model had the special feature of combining

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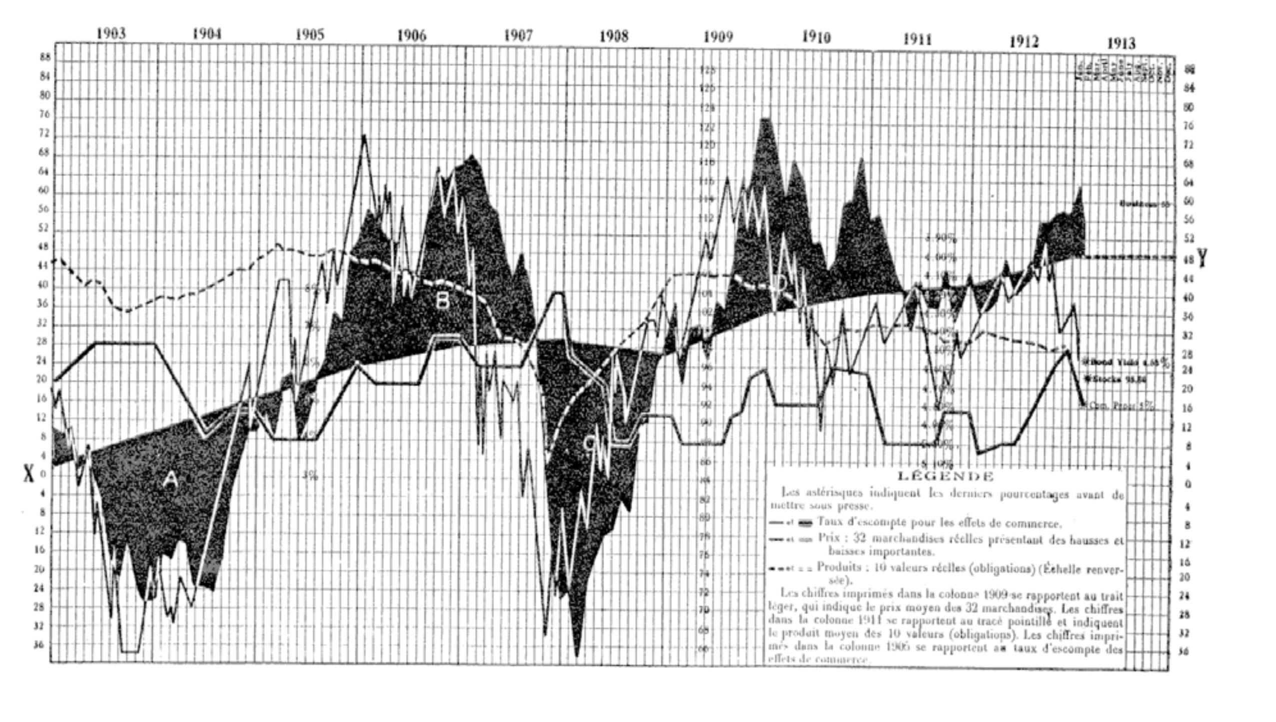
2 Ernst Wagemann was the Director of the Institute for Business Research in Berlin, the largest in Europe since it included about a hundred economists and statisticians just before the second world war. See J. Adam Tooze (1999) on the important part played by the Institute in the Weimar Republic.

3 And which can be illustrated by numerous proverbs, the best known of which in France he quotes: "*Quand le bâtiment va, tout va*" ("If the building is OK, everything is OK").

them by simple addition (after reduction to a common base, the average for 1903 - 1904) into a single index whose fluctuations are arbitrarily intersected by an average line in accordance with a principle which the author claimed to have borrowed from Sir Isaac Newton (who is blamed for so much!). The crossing of this average line should be a simple indicator for the public – "*manufacturers, traders, heads of trade unions and professional people*" – which is the target of the "*Barometer Letter*" and which warns them, for example, "*that business this week is 32 points below the line X-Y, compared with last week's 30 points below the line and the 56 points of a year ago*." (29 August 1922). For its designer this product fills the need for objective information which cannot be provided by the actors themselves, who are too bound up in group strategies **[Fig. 3]**. In the same market there would shortly appear the indexes of Brookmire's Economic Survey which combined 6 factors in accordance with an undisclosed alchemy: speculation, physical volumes, imports and exports, bank deposit movements, discount rate of bills of exchange, and foreign currency rates.

Figure 3: A Babson Chart



4. First statistical tools of barometers

Throughout the period before 1910, the statistical tools used by the creators of barometers were extremely unrefined and were limited to reducing series to a common base, and to a great deal of graph manipulation. The first technique assumes a practice of **synthetic indexes** which was in effect one of the major issues of the statistics of the 1870s. The debate first related to the syntax of these indices, i.e. in essence to the way they were to be weighted: to construct an index for the prices of a group of goods, it seems that in 1738 Dutot, cashier of the Compagnie des Indes, had inaugurated the ratio of means, while in 1764 Carli, an economist and professor of astronomy at Padua, preferred a mean of ratios. In the 1870s Laspeyres and Paasche proposed price averages weighted according to quantity. The average taken can also be varied: arithmetical mean, geometrical mean (Jevons), median (Bowley, Mitchell). Irving Fisher published two works on index numbers (Fisher 1911 and 1922), the second of which explored 134 different formulas.

In relation to the weighting and choice of the sample of goods, Edgeworth, Bowley and Fisher agreed in considering that the former was ultimately secondary. The debate then turned to their semantics, that is to say the theoretical object they were supposed to be representing, namely in the case of a prices index for example, the general movement of prices, or that of a group of goods, or the value of money, etc. Since the work of Jevons and Edgeworth, the probabilistic interpretation of an index has been at the heart of the debate. If one believes the theory of errors and its declension into a theory of averages by Quetelet, then an index is a weighted mean taken on a sample of goods that represents the "true" movement of prices in an economy, just as the average person is the centre of gravity of society. And since this operates to compensate variable and accidental causes, it no longer reflects anything but the effect of constant causes, i.e. the general movement of prices and its counterpart the value of money, regardless of the characteristics of the goods. But a probabilistic interpretation of an average is itself quite daring, both because the conditions of the theory of errors (for example independence, homogeneity and normality of errors) are not respected by price variations, and because there is confusion between price and exchange value. In a lengthy mathematical study in 1909 Keynes himself expressed some reservations about this interpretation, for the same reasons, after having begun to adopt it. Walsh opposed any probabilistic approach even more violently in a controversy with Edgeworth in the 1920s.

The second technique that was blossoming in the 1880s after decades of criticism by adherents of administrative statistics, was the graphical approach. The subject was at the forefront at the jubilee of the Royal Statistical Society, with speeches by Marshall and Levasseur, who introduced the technique very widely in his teaching at CNAM and at the Collège de France. The lectures by Karl Pearson at Gresham College in 1891 dealt with the geometry of statistics and listed a veritable menagerie of all the kinds of graphs. Cheysson explained the graphical method in lectures at the Société Statistique de Paris, and practised it freely at the universal exhibitions of 1867 and 1878 at the Directorate of Maps, Plans and Archives of the Ministry of Public Works, and in a famous article on the associated subject of graphical statistics. Lucien March had a large-format graphical album printed for the exhibitions of Liège and Milan (1907), comprising 273 figures. Graphs appeared fairly quickly as a substitute for tables of numbers, which facilitated inductive inferences. In an example published in 1904 Juglar used a device which illustrated how a table could be converted to a graph and the semantic superiority of the latter **[Fig. 4]**.

Recent studies of the reviews (Kang on the JSSP, Biddle on a selection of reviews) show that graphs were at the forefront as tools and as a subject for the discussion of methodology in the 1910s and 1920s. Works devoted solely to that subject appeared (Karsten 1924). In his treatise, Bowley made of the graphical method an obligatory technique of statistics. He devoted to it a lengthy chapter of about 50 pages, which deals with distribution representations (for example of wages) as well as chronological evolutions. Graphical techniques emerge as an indispensable means for comparative analysis, and as a way "to discover or illustrate causal relationships" (Bowley 1920, p. 153)[4]. Graphical semantics are discussed in detail and even standards for graphical presentation were proposed by the IIS[5]. The discovery by de Foville that the graphical recording of series that are heterogeneous (in their measurement units and variation) suddenly rendered them comparable and enabled them to be combined in a barometer, became the main technique of statistical economics twenty years later.

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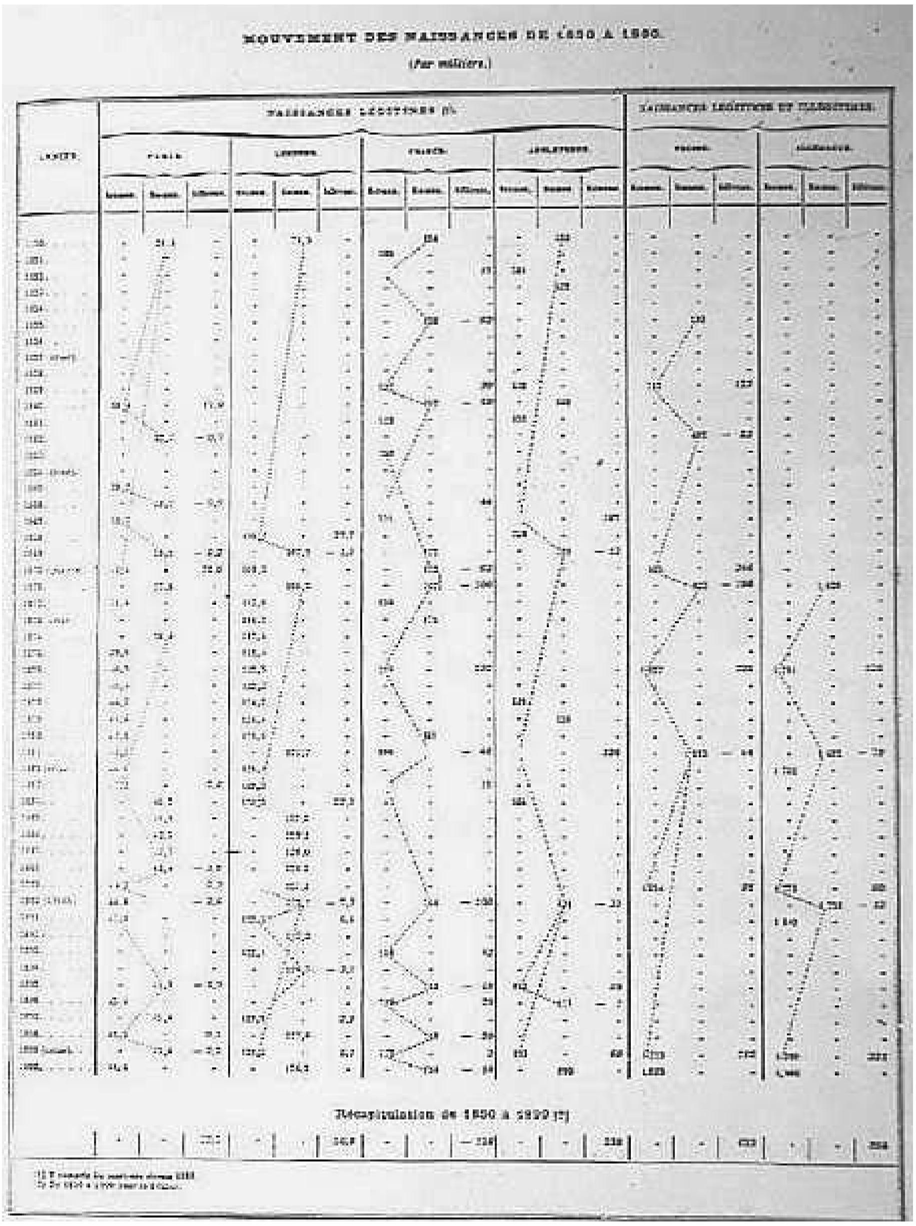
[4] However, Bowley was aware of the difficulties of causal analysis: "*Diagrams may often be used to suggest correlation between two series of figures, and this indeed is one of their chief merits, and they may be used to illustrate arguments on the subject, but at this point their utility ends, for they cannot be made to prove much. Causal relations are very difficult to establish, and the original figures must be critically consulted when theories are to be brought to the test*" (Bowley 1920, p. 155).

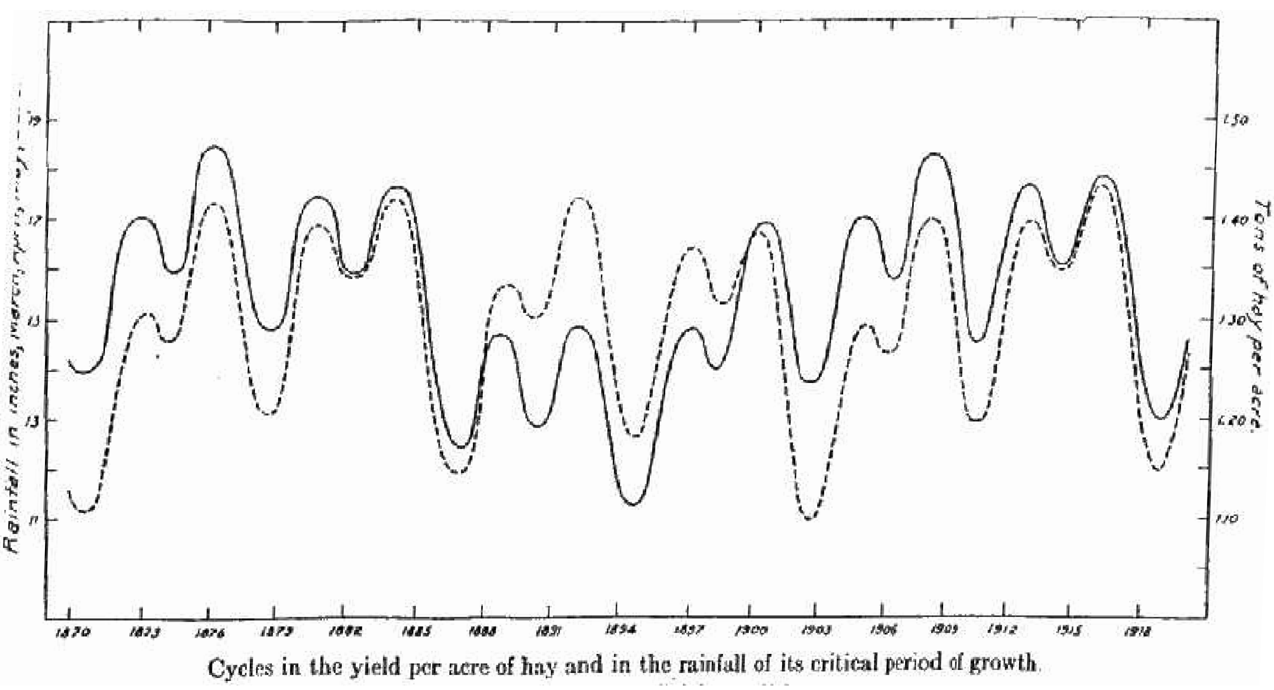
[5] At its 1911 meeting the IIS proposed that "the average of the figures for the years 1901-1910 should be taken as the standard and that this average should be represented by a vertical height equal to the horizontal measurement that represents thirty years" (cited by Bowley 1920, p. 153).

Figure 4: From a table to a graph (Juglar, 1904)



To the technique of indexes and that of graphs must be added the technique of decomposition into Fourier series, which was also fashionable in the 1910s but remained confined to the small group of the few economists capable of carrying out this type of mathematical modelling. The principle is that a function of time can be approached by a limited development which takes the form of a sum of functions of the form *Ai sin(ikt + ei)*, where 2π/i represents the period of the main underlying cycles, ei their phase, and Ai their amplitude. Persons, Crum and Beveridge used this method. H.L. Moor (1914) made rhetorical use of it by showing that the cycle of rainfall and the cycle of harvests a few months later could be broken down in almost the same way, with two major periods of 8 and 33 years, corresponding roughly to the cycles of Juglar and Kondratiev **[Fig. 5]**.

Figure 5: Moore's comparison of cycles



5. The promise of correlation theory

The second barometer period corresponds to the incorporation of new statistical tools imported from English biometrics. Correlation and regression are the two main tools of mathematical statistics that stemmed from the work of Francis Galton and Karl Pearson on the phenomenological laws of heredity, in the context of research marked by Darwinism and eugenics which was very remote or even antagonistic to the preoccupations of economists more reactive to the paradigm of environmentalism. It was G.U. Yule who presented economists – in his fundamental article of 1897 on pauperism, at a key meeting of the IIS in Paris in 1909, and in his manual whose first edition dates from 1911 – with a totally revised version of the theory of regression in which Pearson's idealistic philosophical assumptions are abandoned along with all the apparatus of contingency and the normal distribution, in favour of a working version which ties up again with the lest-squares line of the astronomers and geodesists of the beginning of the 19th century:

> *"To anyone familiar with the theory of errors it will be evident that the method is no more than the application of the well known least-squares method to the objects of statistical research. It is consequently impossible to separate the literature specific to the theory of correlation entirely from literature that touches upon the theory of errors and the least-squares method (…) Since the shape of the frequency distribution given by the law of errors is not common in statistical economics, it is important to obtain the correlation formula and its properties without recourse to the frequency distribution".* (Text in English in BIIS, Vol. XXVIII, p. 537.)

In 1897 Yule demonstrated that regression lines defined as the locus of connected means can be deduced directly from the principle of least squares since the least-squares line coincides with the regression line if the latter is linear (which includes the normal case), and approaches it as closely as possible if it is not. This new approach received the support of Edgeworth in 1902 and 1908 and of Bowley (in his treatise), and it remained only to show its relevance and efficacy in economics. That is what Yule did in his 1909 presentation at the

IIS meeting in Paris, using the now classical examples of his own study on pauperism, the studies of Hooker, Yule and March on the marriage rate and the price of wheat, and Norton's study on the New York money market. That was how most French economists and statisticians discovered the potential of the methods of correlation and regression.

Since then, following the path traced out by these first examples of economics applications, numerous economists began studying correlations between economic series, whether in the context of a partial study or in that of constructing an economic barometer. However, although the promise offered by the tool led to sometimes rather uncontrolled enthusiasm, the difficulties were found to be considerable. To grasp this better, it is quite useful to go into details of the succession of controversies that marked, for example, the numerous studies on the links between economic cycles and demographic cycles.

6. The price of wheat and the marriage rate

The history of the links between demographic and economic variables plays a paradigmatic part in the development of a way of thinking about the links between economic phenomena in terms of new statistical tools, as has been shown by several recent studies[6]. The problem has its distant origin in the theses of Malthus, *whose population principle* asserts that "the available quantity of subsistence goods determines the real population level by constraint". From there he derived a kind of retroactive mechanism in which population and wealth determined one another, a mechanism which he was still trying to make more specific long after the first edition of his Essay on the *principle of population* (1798). Population growth increases the price of subsistence goods and reduces effective income; but that reduction itself affects the mortality, which it increases, so reducing the population (positive braking effect); the lowering of income also affects the marriage rate and fertility, and therefore the population, which it compresses (preventive brake) by compelling couples to cancel or defer their plans to marry and start families. These two retroactive loops tend to stabilise the population at the level of the subsistence goods available, or more exactly those that incomes can afford: "Real salaries and wages are the main regulators of the population and its most exact limiting factor". After Malthus himself, numerous demographers or economists sought to provide statistical validations of the hypothetical causal relationships of this mechanism[7].

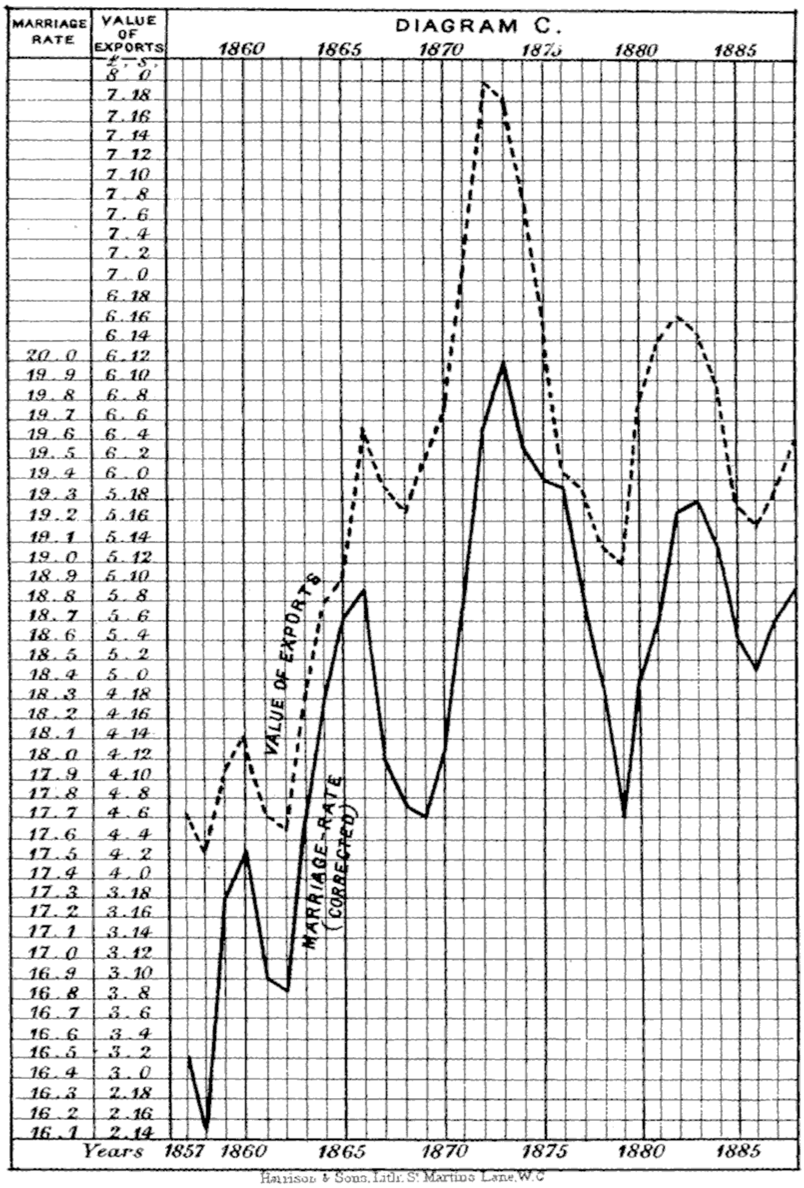
In effect, the first studies carried out at the *General Registrar Office* by its superintendent William Farr (1846) concentrated more on the positive braking effect of mortality, and others took up (without verification) the Malthusian hypothesis of a negative link between the price of wheat and the marriage rate[8]. But William Ogle, Farr's successor at the GRO, published in 1890 a study which took the contrary view based on statistical data from the previous 20 years: "*Indeed the relation between the two is the very opposite. The marriage-rate varies not inversely but directly with the price of wheat*". (Ogle 1890, p. 258). Ogle explained this in terms of a kind of ratchet effect in which a fall in the price of food at first has a favourable effect on marriages, which is soon cancelled by an increase in the "standard of life". The author then went on to show that the series of exports is even more closely sychronous with that of marriage rates.

6 This subject has been studied in more detail by Baumol (1985), Armatte (1992, 2001) and Morgan (1990, 1997).

7 It should be noted, as was done by Wrigley (1984), that this verification is very dependent on the country and time when it is made, and considering that Malthus was writing precisely at the time when the relation between population and wages seemed inverse in England, it is understandable that he was right at the time but wrong later. That is what was demonstrated by data on the period collected by the English historical school in Cambridge and by the work of its *Social Science Research Council* from which Wrigley took his data.

8 For example, Mr.Fawcet wrote in his Manual of Political Economy: "There are few statistical facts better substantiated than that marriages among the labouring class increase with the fall in the price of bread" (quoted by Ogle 1890 and Baumol 1985).

Figure 6: Ogle's comparison of Exports and Marriage rates



Ogle's test is based solely on a graphical comparison, not of the raw series, which gives a graph that is not very convincing, but one that becomes so when the marriage rates are corrected for a tendency estimated roughly as a decline in the marriage rate of 0.47% over 30 years, explained by a shift in the marrying age, a hypothesis considered in detail by Hooker (1898) and Yule (1906) **[Fig. 6]**. The explanation provided for this paradoxical[9] direct relation, involving transport costs, is of little importance here. What is interesting is the use of the graphical test and its sophistication by manipulating the scales, transforming levels into differences or growth rates, and the already apparent separation between tendency and fluctuations.

9 "Men marry, as we have seen, in greater numbers when trade is brisk and when the value of exports increases; but when the exports increase, so also do freights, and this rise in freights causes a corresponding rise in wheat, the largest part of our wheat being imported from abroad" (Ogle 1890, p. 262).

In the first editions of his textbook, Bowley (1901) returns to the example of marriage rate to illustrate the graphical method. Using data covering the period 1860-1900 he makes the series comparable by plotting on the same graph values centred on the mean and reduced to the same unit of variation. From this he concludes that there is an agreement between "marriage rate and trade" but no such agreement between "marriage rate and wheat price", after 1880. His interpretation is directly causal: "*our conclusion is, that since 1870 the causes which affect foreign trade have also affected the marriage rate at the same dates and in the same sense, and that the more marked the effects on the one, the more marked are the effects on the other also, but that there is no law of simple proportion between them*" (Bowley 1901, p. 177). The introduction of correlation tools into the problem of links between economic cycles and marriage rate is also present in the very last part of his textbook[10] and in an article by Hooker published the same year. Bowley computed the correlation between "marriage rate" and "price of wheat" and then between "marriage rate" and "imports and exports", whose values were r = - 0.30 and r = 0.007 for the period 1845 - 1864, and r = 0.47 and r = 0.14 for the period 1875-1894, which illustrates the inversion of the first relation from about 1870 and the non-existence of the second relation[11]. The text read by Hooker to the **Statistical Section of the British Association** in September 1901 and published in the JRSS proposes a different way of handling the same data: instead of decomposing the period into sub-periods in which the connection is almost in the same direction, Hooker reverts to Ogle's idea of decomposing the overall movement of each series into two movements, which he called "trends" on the one hand, and which he calculated with a moving average over 9 years, and what fluctuates around the trend on the other hand. Over the period 1861-1895 the coefficient r = 0.18 found for the relation between marriage rate and exports *per capita* was devoid of significance. If the deviations from tend are considered, the correlation becomes r = 0.80 and even r = 0.86 if exports delayed by 6 months are taken into account. The same method applied to the price-marriage rate relationship confirms Bowley's results, being negative before 1875 (r = - 0.47) and positive after 1875 (r = 0.32).

Hooker's text established a methodology for the studies of time series which become widespread after 1900, until the 1930s. For example, the French statistician Henri Bunle took note of the work of the English school and applied the same methodology to French data (Bunle 1911), and like Bowley and Hooker, found a correlation between marriage rate and the price of wheat which was negative before 1870 but almost zero after that date. Marriage rate, however, seemed positively correlated with prices and trade if the deviations from the moving averages were considered, with a lag of one year for the demographic variable.

7. The notion of correlation in economics

This brief excursion into a case study is instructive. The transfer of biometric tools to the new material of business and the economy is strewn with pitfalls, not to say blocked by an impasse, since measurements of the correlation between two chronological series cannot be interpreted directly. The results clearly differ according to country and period. But they also differ depending on whether the correlation measurement is applied to raw series, to series corrected relative to the trend, or to the trends themselves, not to mention the manipulation of time shifts. What is the sense of each of these measurements? Which of them corresponds to what one might call an explanation? Is it short-term fluctuations or long-term growths which provide us with the key to a mechanism of cycles?

10 In the 11 pages of Chapter 6 devoted to correlation. This part was considerably enlarged in later editions, since in the 1920 edition there are 3 chapters and 60 pages devoted to correlation theory.

11 The test of this value being zero concludes: "the odds against the correspondence between the observed figures, since 1875, arising without causal connection are only about 4 to 1, if we assume that the figures for each year are independent of the next" (Bowley 1902, p. 322). It is true, however, that Bowley adopts a very strict rule for rejecting a causal relation (probable error < r/6). In the 1920 edition, example 6 presents a calculation of r = 0.09 for the entire period 1845-1898 (Bowley 1920, p. 508-510).

The Director of the Statistique Générale de la France, Lucien March, who was very well aware of these difficulties, defended the notion that there is a certain inaccuracy in speaking of a correlation between chronological series. In a text which is often quoted (March 1905) he proposed several measures of dependence the simplest among which is Fechner's coefficient i = (c - d)/(c + d) in which c and d are concordances and discordances of the variation direction of the series. When these are weighted by the product of the standardised variations of the two variables, this produces what is called a *dependence coefficient* which is formally analogous to that of Bravais Pearson. But two differences should be noted. The first is that his coefficient, renamed *coefficient of differential co-variation* in March (1928), does not relate to the series in terms of level but to their primary differences. It is of course easy to show series whose level correlation, which he rebaptised *tendential co-variation*, is positive while the differential co-variation is negative. We then revert to the separation asserted by Hooker between trend and fluctuation, but with a filtration method which is no longer that of the deviation from tend (obtained by the moving average) but that of the primary differences. The second discrepancy claimed by Lucien March between the notion of correlation in biometrics and that which should be built up in the economy of cycles relates to the term *co-variation* itself, which Armand Julin took up on his own account and explained perfectly clearly in his treatise:

> "*With Mr Lucien March, we regret that this term [correlation] has become part of the vocabulary of statistics by virtue of its meaning in logic and in ordinary language, a meaning that it does not have in statistics. When one says that one fact is correlated with another, one usually wants to express that one of the said facts stands in such a relation to the other that one assumes the other (Littré). In logic the correlative nature of two phenomena implies the existence between them of a causal link (…) The name of co-variation, which implies only that there is a concomitance between the variations of the phenomena, is proposed and used by Mr March and we adopt this terminology*." (Julin 1921, p. 479-80).

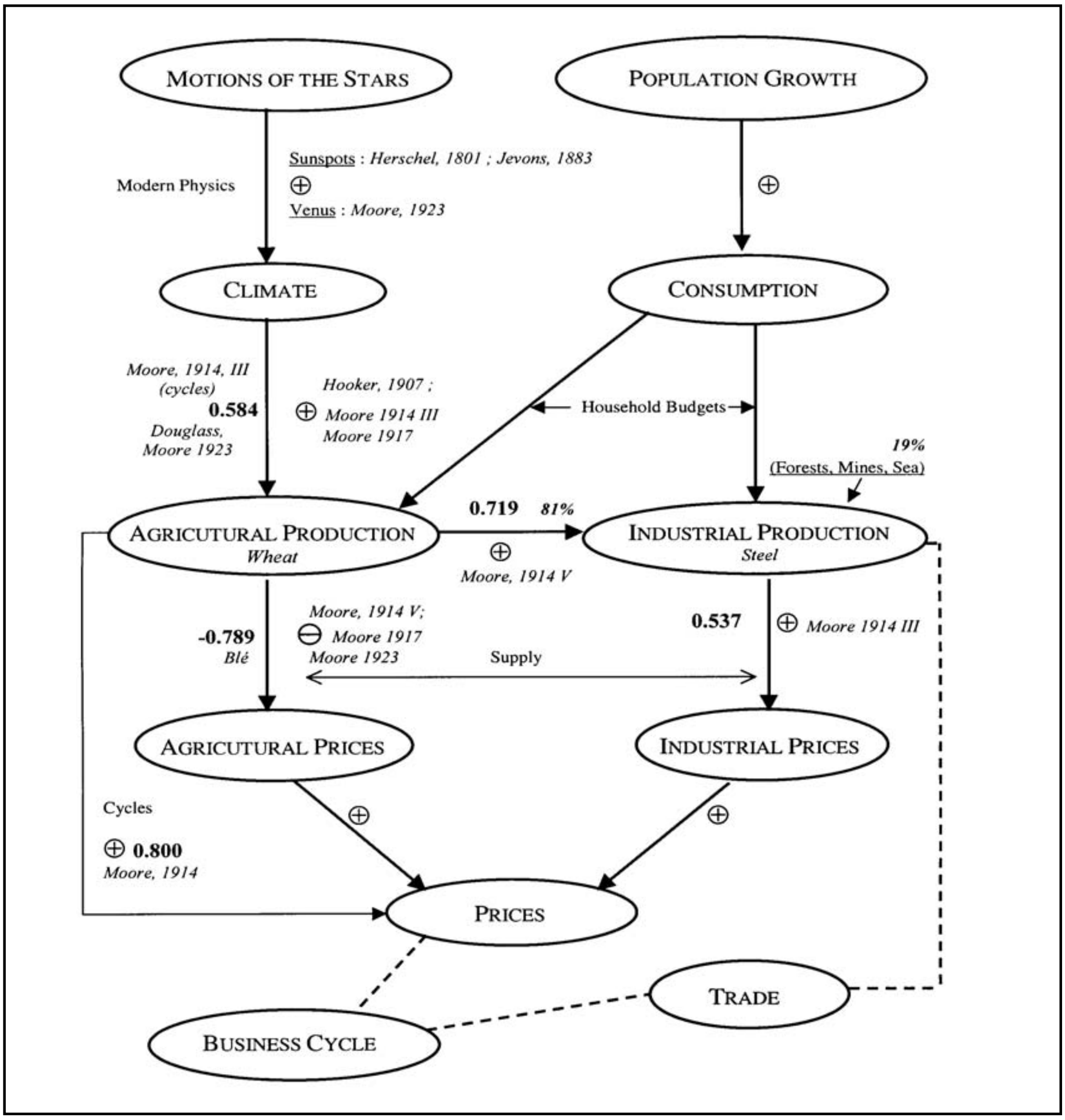
The difference from the English usage of correlation is dual. On the one hand it denies a causal interpretation of correlation, according to which the correlation stems from the existence of common causes or would even be the expression of a balance between common and distinct causes, as Bravais had shown and as Bowley reasserted in his treatise. It is not surprising that March, who translated and published in 1912 the *Grammaire de la Science* (Grammar of Science) which disseminated in France its author's idealistic philosophical thinking at the same time as his eugenic theses, should become like him the defender of a science in which any mechanical notion, beginning with that of cause, had to be discarded in favour of the broader and less compromising concept of contingency. Thus, the coefficient of co-variation is a simple measure of a contingent relation located somewhere between simple concomitance and correlation interpreted in a causal way. This return to Pearson is also a defence of the specificity of the field of economics, and more particularly its place in history. The correlation of biometricians expressed a synchronous relation between characteristics of two organs measured at the same time in the same individual, or in two related individuals. For Pearson chronological series are never observed, only instantaneous distributions. Everything changes in the economic study of Business cycles for which time, in contrast, is one of the most important factors. As Mary Morgan (1997) says clearly, the notion of correlation in economics very often assumes a search for an explanation in which the concept of cause signifies both precedence in time and location in a historical context that renders it sometimes operative, sometimes not, and always interwoven with other causes.

The notion of correlation in Business cycles is therefore completely located in a historical vision which comprises both the term "co-variation" and the chronological graph of indexes and barometers from which it is inseparable. The graphical recording of concomitances must be made explicit and objective by a correlation calculation, but that correlation calculation needs the graph for its interpretation. As Morgan also notes, and as a result of the interpretation difficulties which have been stressed, the measurement of correlation has not at all supplanted graphical representation, has not at all gone beyond it, and has not been established as an autonomous tool. This is because the coefficient of correlation still indicates nothing more than a co-variation. It should be noted that the same does not apply in a different branch of economic theory in which correlation has played a major part, namely the theory of demand. In that case the relation between prices and quantities has adopted a connotation closer to that of biometrics because, even for data in series, clouds of points (scatter diagrams) supporting the search for a functional relationship were quite soon substituted for chronological graphs, though not without difficulty since it was necessary in that context to link on the one hand the two interpretations of co-variation and the course of the data, and on the other hand a structural connection which was non-historical and could be interpreted in terms of a law of demand *ceteris paribus*. While the empirical law of demand played the part of a bridge between these two universes, allowing an appreciation of the difficulty of articulating them (the problem of identification, for example), the barometer is an object entirely immersed in the universe of co-variation alone. Thus, the causal interpretation of simultaneities is based solely on the temporal order and direction of variation, but cannot be expressed by a stable, non-temporal functional relationship that could be called a law of economics. This, no doubt, is one of the reasons why many economists continue, as opposed to this bashful and maltreated causality, to prefer the semiological, quasi-medical interpretation of co-variations discussed to begin with.

Nevertheless, other economists radically committed to a usage of correlation which seems promising, but sometimes goes beyond its inferential possibilities. On the basis of these techniques of decomposition, linear adjustment and correlation, the methodology of the treatment of chronological series seems to deviate very significantly and even to depart entirely from the central concept in which barometers are regarded as simple aggregates of symptoms, reduced only to a semiological function. An author such as Henry Ludwell Moore, an American economist trained in statistics by Karl Pearson, undertook a series of studies which no longer spoke of barometers, heuristics and forecasting. They sought to find true mechanical models of an economy which would explain both its growth and its cyclic fluctuation, because they identified its workings in the form of strong links between variables, attested by high correlations, and operating as conveyor belts. These links may correspond to laws of economic theory established in unrealistic, *ceteris paribus* conditions, or they may deviate from them to foreshadow a different kind of dynamic theory, but one whose essence is grasped by correlation. Without going into details of what is no longer in the field of barometers, an overall view of such a mechanism can be obtained by putting together the few studies published by Moore between 1911 and 1923 in the form of a summary graph of the relations he found between price movements, agricultural production, climate variations and the motion of the stars **[Fig. 7]**.

This usage of correlation bears witness to a slide from contingency towards causality which Mary Morgan has also noted: "*The correlation coefficient, as a loose associative notion depending on common cause arguments in the temporal domain, can easily be reinterpreted to serve as a measure of a direct cause-effect relationship which dispenses with both the notion of common causes and history contingency. We can see both these types of usage of correlation in Persons's own work*." (Morgan 1997, p. 72).

Figure 7: Moore's causal explanation of Economic Cycles.



8. Second-generation barometers

Warren Persons is in effect another American economist who chose to develop the notion of a barometer with the aid of correlation tools. The promoter of a *Committee for Economic Research* at Harvard in 1917, he was above all the inventor of the famous Harvard Barometer which can be regarded as the prototype of a new generation of barometers. Persons's main idea was that there must be a division of labour in the work of business analysts, between the ones who publish data and those who analyse the data. Criticising in barely veiled fashion the semi-charlatans who proliferate in the field by publishing raw or poorly treated data, he clearly located himself among those capable of mobilising the new statistical tools. In the structure of his barometer, however, the dual influence of "chartists" and mathematicians can be perceived.

His barometer was published in the first issue of the *Review of Economic Statistics*, which he founded (Persons 1919). In its first version it was constructed from 20 series chosen from among 50 series found in government publications. For each of them, the trend is determined by a linear adjustment of annual data. Seasonal indexes are then calculated by the method of linking trend relatives, by taking the median of these relatives and then linking them. The original monthly data are then corrected for trend and seasonality, and then reduced to the same dispersion unit. The twenty cyclic series so obtained are then systematically studied in pairs to determine their displacement and correlation. The method is first graphical: three independent observers observe the optimum coincidence of the series reproduced on transparencies with the aid of an illuminated table, and note both its optimum displacement and the quality of correspondence, using a summary scale (excellent, good, acceptable, poor). This first evaluation is supplemented by calculating the linear correlation between the cycles and the displacement that maximises this correlation, a calculation necessary when the said displacement varies throughout the period. The correlation is deemed "absolutely certain" if its absolute value is higher than six times its probable error, and "striking" if it exceeds 0.5. The series are then sorted and first divided into five groups, then into three, as a function not of their nature, which could have been done *a priori*, but as a function of a similarity of their cycle, i.e. of the correlations and especially the "lags" calculated, leading to the famous system of three curves by which the Harvard barometer was popularised. The nature of these three classes was in some way discovered *a posteriori*: group A (4 series) is an index of speculation, group B (5 series) a combined index of physical productivity and prices, and group C (4 series) an index of the financial situation in New York. Thus, each of the curves describes the movement of one of the three markets: financial market, goods market and money market. The three series seem to follow the same movement, supposedly that of BCs, with series A anticipating this movement and being therefore regarded as an advance index and support for forecasts **[Fig. 8]**.

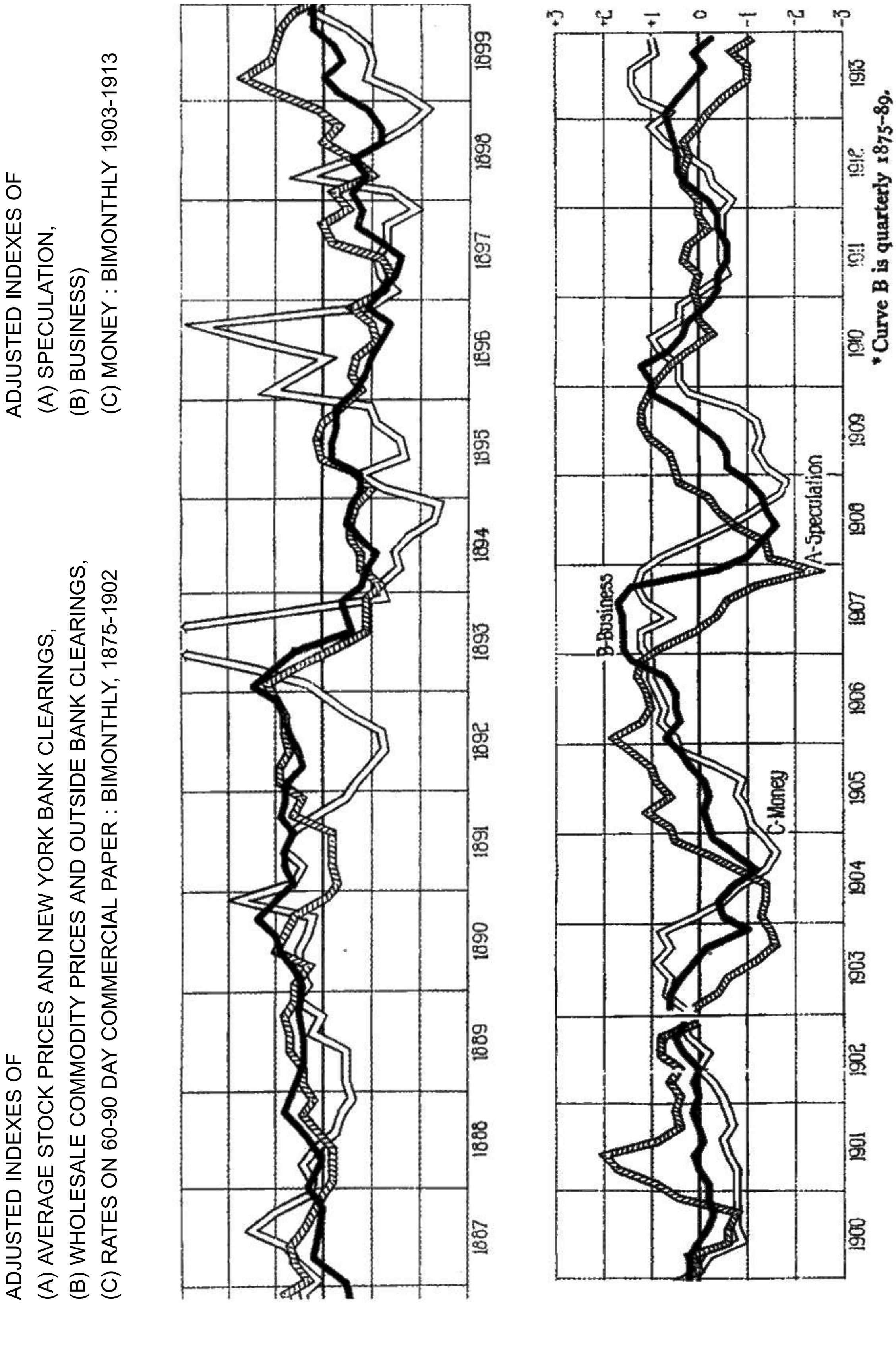
The composition of the Harvard barometer was to undergo many modifications in the years to come. In 1928 curve A was finally reduced to two series of indexes: the prices of industrial shares (Dow Jones) and the prices of railway shares; the Bureau of Labor prices replaced Bradstreet prices in curve B, and curve C no longer contained anything but the long-term discount rates and railway bonds. The correlations between series and the essential relation between the three curves, estimated over the period 1903-1914, was at first satisfactory and even very successful. For example, the 1920 crisis was clearly predicted by Harvard, although its amplitude was underestimated.

The success of this barometer was so great that numerous imitations of the device appeared in Europe, by Beveridge in Cambridge, March in Paris, Dupriez in Louvain, Wagemann in Berlin, and Kondratiev in Moscow. The idea that these tools could also allow public authorities to act well-advisedly upon the levers of economic policy also emerged. For example, in a BIT report of 1924 one reads:

> "*If businessmen can almost unconsciously influence the future situation or small groups of speculators can deliberately provoke the appearance of conditions favourable to them, one imagines it should also be possible to influence the future in the interests of the community as a whole*."

The methods of economic analysis and forecasting were institutionalised at business Institutes which enabled them to survive the crisis. In France, for example, it was on the day after the crisis of 1907 that a Commission was set up, entrusted "to study the measures to be adopted in order to reduce the unemployment resulting from periodic economic crisis", followed by a permanent committee whose lines of credit were only opened in 1914. The *Service d'Observation des prix* (Prices monitoring service) was not officially created until 1917 and attached to the SGF in 1919. Numerous indexes have been published since 1911 by the SGF in its bulletin, on the occasion of certain exhibitions (Ghent 1913), and in the Revue politique et parlementaire since 1919. The monthly publication of a collection of "Indexes of the general movement of business" fell in 1922, at the same time as the foundation of the ISUP, to Lucien March who was no longer directing the SGF. It took the form of about ten large-format plates, the first describing the evolutions of three indexes (the Harvard types A, B and C) for France, the United Kingdom and the United States, while the other plates represented about fifty French indexes (finance, domestic and foreign trade), the whole accompanied by a note on methodology and a business commentary.

Figure 8: Harvard Barometer



Technically, the indexes are worked out much more simply than in the Harvard case: they are expressed as "values relative to a monthly average calculated over the period 1904-1913", which eliminates neither the trend nor the seasonality. On the other hand they are not the result of groupings mechanically operated on the basis of the correlations observed; in most cases they are only simple ratios, averages or budgetary indexes. These indexes (*Indices* in French) would according to A. Sauvy (1954) not be very useful except for "*an unexpected application: the use of price and cost of living indexes for the execution of index-linked contracts. A mechanical use applied without discrimination, which led to countless court cases*".

He prefers private initiatives such as the single index known as the "industrial barometer" published daily from 1923 onwards by the *Société d'Etude et d'Informations Economiques*, or the analyses of Jean Dessirier who left the SGF to create a monthly publication, the Conjoncture économique et financière, which favours less fully equipped, less automatic and more critical methods. Charles Rist's *Institut scientifique de recherches économiques et sociales* (Scientific institute of economic and social research) also publishes a quarterly review – *l'Activité économique* – to which the business analysis studies refer.

9. Economic crisis, crisis of the barometers, new perspectives

After 1924, however, the curves of the Harvard barometer diverged: shares took off and prices stagnated. In his course of 1928, published a few months before the stock-market crash, Professor Aftalion nevertheless remained confident: "*In 1925-26 we witnessed this new phenomenon: a fall in prices together with a rise in production. Thus, curve A has not positively failed in its predictive role. Although it has not foretold a crisis, to tell the truth no crisis has taken place although there was something of a slowdown in 1927-1928*" (Aftalion 1928, p. 276).

Besides, almost everywhere in Europe it was the crash of 1929 and the crisis of the 1930s which dealt a fatal blow to barometers while at the same time encouraging thoughts about State regulation and intervention, the publications of X-Crise and especially the *Bulletin du Centre Polytechnicien d'Etudes Economiques* bearing witness to this in France between 1932 and 1939 (Armatte 2001). In effect, these bear the marks of a lecture on "indexes of statistical movement" by Dugé de Bernonville (13 December 1935) and another by Wagemann (CPEE, November 1937), and a regular business column written first by Dessirier and then by A. Sauvy, who made it both a summary of the state of the economy and a mixed platform for the criticism of economic policy (the 40 hours, for example) and for the promotion of his own ideas[12].

The barometers amassed a great deal of empirical knowledge about economic cycles. But they are symbols of the failure of an attempt to establish a reliable forecasting method. An economic crisis is therefore also a crisis of methodology. The die had been cast: there was no talk of barometers after 1930 except to criticise them, but as Sauvy said (1954, p. 92): "*One is therefore inclined to reject definitively, as museum pieces, the old business barometers used between the two wars. One should be wary, however, of selling one's horse before the machine called upon to replace it has been properly developed*". So what was to replace barometers? Four different tendencies can be perceived in the answers offered to that question in the 1930s and 1940s.

[12] A. Sauvy, "A monthly index of the cost of living in France", *Bull. 37*; "Artificial economic revivals", *Bull. 39*; "Financial crisis and population crisis", 7 February 1939, *Bull. 55*. See also the contribution by Sauvy published in *On the recurrence of economic crises*, Economica, 1982, *his Essay on business analysis and economic forecasting* published in 1938 as document No 8 of the CPEE, and What do I know which he subtitled *on economic forecasting* (first edition, 1943). However, it was not until 1938 that a true Institute of business analysis, directed by A. Sauvy, was created in an administration. Attached in 1941 to the National Statistics Service which formed the transition between the SGF and INSEE, it disappeared in 1946 and its functions were shared out between INSEE, the Commissariat for Planning and the Department of Economic and Financial Studies (the future Directorate of Forecasting).

The first comes from business analysts themselves, who see these statistical treatments as a misleading manipulation of economic data, a cloud of smoke and dust in the eyes, cleverly disguised as mathematical science, which hides the reality of phenomena more than revealing it. They want quite simply to return to the art of business analysis, an art close to that of practitioners which is not cluttered with sophisticated tools. Partisans of kind of chartism, they criticise the mechanical use of barometers and claim a return to the semiological concept. This point of view is well represented by Jean Dessirier in Paris and Wagemann in Berlin:

> "*These curves cannot ultimately shed light on the mechanism of economic cycles, nor constitute a kind of barometer as is sometimes believed: (…) the apparent automatism is purely illusory … More generally, would it not be appropriate to react against a certain tendency that consists in systematically squeezing economic phenomena into definitions and mathematical formulas that are rigid and much too narrow? (...) The whole art of the statistician, the whole difficulty of economic forecasting reside in this "feel", this flexibility, this updating and infinitely sensitive analysis of the most useful elements, much more than in the search for ideal mathematical formulas capable of apparently replacing a part of judgement and experience*." (Dessirier 1928).

> "*One of the greatest mistakes of the American business analysis service is to have worked mechanically in accordance with general formulas. This led to a complete fiasco when it was claimed that the American economy had been correctly assessed during the upswing period of 1927-1929 (…) The purely mechanical application of this method cannot be enough; it is only an aid to the solving of problems, just as medical diagnosis only provides a basis for assessing the patient's condition*." (Wagemann 1938).

> "*The system of functional relations is subject to the organic-biological principle of economic dynamics. By this, I mean the fundamental conception of the economy as a living organism, analogous to those of animal and plant life from two points of view: 1. Close association of all the parts that make up a closed system governed by its own laws. 2. Effect of external influences manifested only in the form of stimuli which trigger movements, these being subject to the autonomous laws of the economic organism*." (Wagemann 1932, p. 36).

Jean Lescure, professor in the faculty of law, in a controversy with the statistician Bunle over the question of whether economic studies should be made autonomous in relation to faculties of law, is in the same, much more reactionary vein in the primary sense of the term; he advocated a return "to the diagnostic methods used by doctors" on the basis of the actual figures "without correcting or manipulating the figures", without constructing any index and without eliminating either the trend or the seasonality. So to speak, a return to the stone age:

> "*The tendency here is all too often to* ***misuse indexes****. The figures themselves are in many respects more instructive than an index (…) Similarly, it is common nowadays to "weight" or "adjust" indexes. We now express the most explicit reservations about this way of* ***manipulating the figures***."

> "*Rather complicated methods have been thought up which enable statistics to be corrected, expurgated in some way, so as to eliminate the influence of long-term and seasonal variations. These would only seem to us to be worth recommending if it were possible to apply the spirit of geometry to the observation of economic phenomena, if it were possible to compare one cycle with another as one compares triangles or angles. But a detailed historical study of these phenomena shows that* ***every cycle has***

> ***its own particular features**, that although cycles may be analogous they are never identical (…) To eliminate by calculation what it has been agreed to call the trend, is to mask one of the essential characteristics of economic evolution. It is to commit an error analogous to **that of a doctor** who would care for his patient without regard to the patient's general condition*."

One could also quote Baron Mourre, who considered that his "forecasting system [published in 1913] was along general lines the same as that of the University of Harvard's committee and only differed from it in some accessory points", among which he included "the elimination of the secular trend [which] is only of very little use in the forecasting of crises (…)" and "the elimination of seasonal variations" (Mourre 1928, p. 281-282).

The reluctance of the NBER's institutionalists about statistical manipulation and the mechanisation of barometers was not so great, but led its main inspirer, W. C. Mitchell, to return in the second edition of his work on Business cycles (Mitchell 1930) to the credo of the first edition of 1913: Every cycle is unique and, what is more, irregular, which calls for detailed analyses, and which led him to reject over-simplistic periodic models. He was not even quite sure that the Business cycles, as such, had any conceptual reality. He listed most of the statistical works on barometers with a good knowledge of their institutional roots but with poor technical understanding (which caused him to confuse R and R2). More interesting is his comparison of five American indexes, which enabled him to set up a kind of typical picture of the cycle and to introduce the NBER's own method, that which he was to develop after 1935, which was published in 1946 by Vining and which triggered the anger of Koopmans. This approach aims at a classification and description of cycles from a simple, morphological point of view. It starts from observed series, looks for their type in the form of a reference cycle described in detail in 8 symmetrical phases and broken down in various shapes – rounded for prices, triangular for production, dotted for speculation – and allowing identification, by comparison with this reference cycle, of advance or lagging indicators that may be pertinent for business analysis. Although not statistical, this is indeed a totally empirical approach but one that distances itself from the techniques of statistical reduction and from the automatic forecasting aimed at by the barometers, of which Mitchell is fairly moderately critical.

The statistical techniques employed by the barometers are undermined much more by the work of mathematicians and statisticians who decry the erroneous use of correlation measurement and the artefacts that this measurement can produce. The most incisive criticism comes from the famous presidential address by Yule at the RSS meeting in November 1925 (Yule 1926), which dealt with *nonsense correlations between time-series*. Yule put forward the famous example of the correlation between the time series of mortality in England and the proportion of marriages celebrated by the Anglican church. Now, that very close correlation (0.952), although validated by the usual r significance tests, is clearly meaningless in the sense that one cannot imagine any direct connection between those variables. A series of papers by Yule (1921, 1926, 1927) and by Slutzky (1927) shows that the main decomposition methods used – moving averages, differentiation, harmonic decomposition – have an annoying tendency to create artificial correlations or cycles. Yule had already pointed to the role of the self-correlations of each series in the final result of the measurement of a connection between two series (Armatte 2000). Finally, a campaign carried out in 1934-1936 at the International Institute of Statistics by the mathematician Maurice Fréchet violently attacked the mistaken use of the correlation coefficient wrongly enlisted in mixed operations to measure the dependence level and the linearity of that dependence, pointing out that the value zero is in no case synonymous with the absence of a connection, while the value 1 only means that there is marked linearity. Via a broad survey among statisticians all over the world and motions passed at two IIS meetings, but also via the mathematical work of the Italian school (Gini, Pietra) and his own work on the notion of distance, Fréchet sought to make more relative the excessive significances yielded by the correlation coefficient. Its maximisation, widely used to determine the lag between cycles of the series in barometer construction, was the direct aim of that campaign (Armatte 2002).

A fourth and last reaction also started from the failure of the statistical method used by barometers, and complained that its result was a methodology founded solely on inductive inference and the consideration of repeated regularities, a method which has shown its limits in economics as in the natural sciences (see Bode's law in astronomy). But this criticism, poles apart from that of the business analysts, tried to be positive and called for yet more theory and yet more mathematics. It was supported by the econometrists who met in the *Econometric Society* founded just after the crash (30 December 1930), and even more among the small group of the Cowles Commission founded two years later. They called for an articulation of the deductive method (mathematical economics) and the inductive method (statistical economics), and sought several alternatives to the construction of barometers. The first was that of the small oscillator models proposed by Frisch, Tinbergen and Kalecki between 1933 and 1936, which sought an explanation of cycles in terms of a dual mechanism combining impulses and propagation in a device with its own dynamic characteristics. The second form of modelling is that of stochastic structural modelling that would constitute the paradigm of econometrics for over thirty years. At the core of tthis methodology, which was made quite explicit by the "manifesto" of Haavelmo (1944), the notion of a stochastic structural model replaces inductive statistical inference based on co-variations, a hypothetical-deductive approach based on "autonomous" relations made explicit by equations that are both functional and stochastic, and a new statistical tool: the hypothesis test.

Curiously, however, it was the work of Tinbergen and his macro-econometric models of the Netherlands (1936) and then the USA (1939) which imparted a certain visibility to the econometric approach. Now Tinbergen, who was fairly distanced from the small Cowles Commission circles, was trained in business analysis at the *Central Bureau of Statistics* which he entered in 1928, via the practice of barometers – of which, it is true, he was rather critical – as initiated by M. J. de Bosch Kemper, who adapted the Harvard example to the conditions of an open economy[13]. According to van den Bogaard (1999, p. 301) since 1935 he had expressed the need to replace the barometric approach by the use of "quantitative schemes" expressing a theoretical economic structure in the form of dynamic relationships between certain aggregates and predetermined variables: "*In order to know the eigen-movements we have to construct a model which consists only of regular relations*". It was, besides, to test the theories of BC that he was recruited by the Society of Nations, an unexpected context for the publication of his model of the economy of the United States. After he too had tried out highly theoretical models inspired by theoretical physics and then engineering science, he reverted to statistical tools, which play a dominant part both in the specification of the model and in its appraisal, to the point that he gave the name "method of multiple correlations" to his methodology set out in the first volume of the report of 1939 (Boumans 1992, Armatte 1995). Thus, in Tinbergen's macro-economic model there are many elements of continuity with the barometer method: the same objective of forecasting and action upon an object of simulation with a view to effective economic policy, the same obsession with cycles, the same idea of temporal co-variation, a central utilisation of correlation and graphs. These elements are besides the ones that the Cowles Commission would want to eliminate by appealing more for the introduction of an a priori structural model based on relations assumed to be autonomous, and on the range of probabilistic hypotheses that enable confusion between the validation of that probabilistic model by statistical tests and a validation of economic theory.

13 This Dutch barometer comprised 4 series (instead of 3 for Harvard) - the markets for goods, securities, money and labour - represented separately (like the indexes of Lucien March) but superimposed on foreign series (American, English and German).

Nothing, however, is further from the philosophy of barometers than this emphasis on a theory, a separation between exogenous and endogenous factors, the expression of organic connections by functional relations, hypotheses that could be specified *a priori* and tested *a posteriori* by bringing in the calculus of probabilities. There, we are in another world.

10. Conclusion

For about twenty years barometers were the favoured framework of comparison between the various economic methodologies of the years between the wars – morphology, semiology, harmonic analysis, correlation analysis – and they have continued to be a reference – positive or negative – for modelling in terms of oscillators or in terms of structure. They constituted the favoured territory for the importation of the main tools of mathematical statistics into economics. For that reason they compelled economists to reconstruct the significance and efficacy of those tools. They provided a good empirical knowledge of cyclic phenomena, but did not allow the testing of competing economic theories on cycles. They failed to provide sufficiently reliable forecasts, and even more to allow control and regulation of national economies. Discredited by the major crisis of 1929, they served as a stimulus to the design of a new econometric approach which also integrated the a priori knowledge of economic mechanisms more effectively. It is not without malice that, to end with, we recall the reappearance at the end of the 1970s of their philosophy of a non a priori treatment of chronological series (for example, in the form of VAR models), when structural modelling began to lose its allure under the impact of the oil shock and the overlapping criticisms of Lucas and Sims.

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EUROSTAT COLLOQUIUM

HISTORY OF BUSINESS CYCLE ANALYSIS

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KONDRATIEFF
OR HOW TO CONVINCE PEOPLE THAT
LONG-TERM ECONOMIC EXIST

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The *Kondratieff cycle* has been the unit for measuring economic time ever since Joseph Schumpeter[1] gave the name of the Russian economist to the long cycles of economic development.

Schumpeter thereby brought to a close a dispute on the existence of possible economic movements of longer durations than those postulated up to then, which had been identified by Juglar in 1860. Meeting Marjolin's doubts[2] on the issue head on, he recognised the reality of them in response to the significant amounts of statistical material gathered and the agreement among numerous publications.

From the end of the 19th and the beginning of the 20th centuries, research into the conditions of economic expansion have revealed "undulations" of long duration. In his study of the industrial crises of England[3], Tugan-Baranovsky detected a succession of periods during which industry develops faster, the process of which could not be captured by traditional crisis theory[4] and Van Gelderen sought an explanation for the causes of the changes in the "purchasing power of money"[5] to determine different periods in the evolution of capitalism[6]. After the First World War, the number of studies grew, and gradually the vocabulary in use evolved: the writers made fewer and fewer references to metaphors related to movements of the sea (as Van Gelderen had done[7]) and turned to using phrases expressed in terms of cycles (Cassel[8], Dupriez[9], Spiethoff[10], Wageman[11], De Wolff[12], Woytinski[13]) starting out from ideas expressed on the theories of over-capitalisation or quantity theory of money.

While, by choosing the name of Kondratieff over that of any other authority to describe this type of "cycles[14]," Schumpeter did cause the name to go down in history, at the same time he eliminated Kondratieff from memory as an economist of originality and independence of thought.

Soon, nothing was left of him but "his" famous statistical method of *trend-deviation*. His analysis of "movements in the form of sing and falling waves" in economic activity was rapidly reduced to a semantic

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1 Schumpeter J.A., *Business Cycles*: A *Theoretical, Historical and Statistical Analysis of the Capitalist Process*, 1939, 1st edition, 2 volumes, MacGraw-Hill Book Company, New York, Toronto, London.

2 Marjolin R., 'Mouvements de longue durée de prix et extraction des métaux précieux,' in: *L'activité économique*, January 1937 and 'Rationalité et irrationalités des mouvements économiques de longue durée,' in: *Annales Sociologiques*, series D, book 3, 1938, pp.1-38.

3 Tugan-Baranovsky M., *Les crises industrielles en Angleterre*, 1913 (translated from the 2nd, revised and amplified, Russian edition), Giard & Brière, Paris, 476 pages.

4 'The periodic return of epochs of prosperity and of crisis is a characteristic feature of the capitalist mode of production. But these fluctuations must not be confused with the changes in the situation of industry which extend over extremely long periods,' ibid., pp. 32-33.

5 Gelderen J. (Van), 'Springvloed. Beschouvingen over industrielle ontwikkeling en prijsbeweging,' in: *De Nieuwe Tijd*, vol. 18/1913, no. 4 (April), pp. 253-277, no, 5 (May), pp. 369-384 and no. 6 (June), pp. 445-464.

6 'If we consider the stages of the movement of prices (…) as manifestations of the different phases of the capitalist development, the knowledge of the expensiveness will also mean the knowledge of the essence of capitalism,' ibid., p. 253.

7 The title of his article referred to above does indeed translate as '"Spring Tide. Considerations on industrial development and the movement of prices.'

8 Cassel G., *Theoretische Sozialökonomie*, 4th edition, Leipzig, 1927, XIII-649 p..

9 Dupriez L.H., 'Einwirkungen der langen Wellen auf die Entwicklung der Wirtschaft seit 1800,' in: *Weltwirtschaftliches Archiv*, Vol 42, no. 1 (July), 1935, pp. 1-12.

10 Spiethoff A., '*Krisen, in: Handwörterbuch der Staatswissenschaften*, Vol 6: *Kriminalstatik- Reklamesteuer*, Jena, p. 8-94, 1925 (1st edition: 1923).

11 Wagemann E., *Struktur und Rythmus der Weltwirtschaft. Grundlagen einer weltwirtschaftlichen Konjunkturlehre*, Hanseatische Verlagsanstalt, Hamburg, 1931.

12 Wolff S. (De), 'Prosperitäts- und Depressionsperioden,' in: Jenssen O. (ed.) *Der Lebendige Marxismus, Festgabe zum 70. Geburstag von Karl Kautsky*, Jena, 1924, pp. 13-43.

13 Woytinski W., 'Das Rätsel der langen Wellen,' *in: Schmollers Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft im Deutschen Reiche*, Munich and Leipzig, 55th year, 1931, no. II, pp. 577 and ff.

14 He explains in *Business Cycles* that 'it was N.D. Kondratieff however, who brought the phe-nomenon fully before the scientific community and who systematically analyzed all the mate-rial available to him on the assumption of the presence of a Long Wave, characteristic of the capitalist process,' op. cit., volume 1, p. 164.

argument on the terms to be used (cycles, not cycles?) or an existential argument about empirical verification of them, and became more or less identified with what was, in fact, only Schumpeter's interpretation of the long term.

Kondratieff's original goal was that of a rural economist: to stipulate the conditions under which the price ratios between agricultural products and industrial products evolve, in order to determine the framework of agrarian policy in Russia. It was in this context that he concentrated on analysing the different types of economic business cycles and undertook his research into the "large cycles within business."

In this paper we shall primarily be concerned with Kondratieff the economist. His working life as an economist was short: from 1920 to 1928, the year in which he was dismissed from office before being interned from 1930 to 1938. For all the brevity of that period, he left behind him an important and closely-argued body of work, ignored or barely known for too many years and consequently largely misunderstood. Today, a significant portion of his writing is finally accessible[15], making it possible to reevaluate it and begin to present his vision of the large economic cycles in a more coherent manner.

This paper sets out to synthesise Kondratieff's work, in order to offer an interpretation of the way in which he described the internal workings of the large cycles. It will be seen in the course of the paper that the approach here is far from mechanistic and that from many points of view, Kondratieff appears modern.

In the first part, we shall present his intellectual and professional journey and describe how his writings reached the West.

The second part describes the methodology established in order to verify the hypothesis of the existence of "large cycles within business."

And the final part will be devoted to reconstructing and critiquing the theory developed by Kondratieff on the basis of relations between agriculture and industry to explain the regular alternation of long periods of expansion and recession.

1. The intellectual journey of Nikolai Kondratieff and the problems of distribution of his writings

Studying the evolution of long-term trends in the business cycles of the economy was in fact a methodological detour from the primary object of Kondratieff's research, namely the conditions under which the agricultural production of Russia, and then of the Soviet State, could be profitably utilised.

1.1 Nikolai Dimitrievich Kondratieff was born in 1892 into a peasant family living in a village about 300 kilometres north of Moscow. Following brilliant secondary studies, in 1911 he was admitted to the Faculty of Law of Saint Petersburg where he studied under M. I. Tugan-Baranovsky and the economic historian V. V. Sviatlovskii. In 1915, he submitted his dissertation, which aroused considerable notice, devoted to the '*Economic development of the Zemstvo of Kinechma in the province of Kostroma*.' This in turn allowed him to seek election to the professorial chair of political economics. Passionately involved in mathematics and formal logic, he became head of the *Department of Economic Statistics of the Union of Zemstvos of Saint Petersburg* at the age of 24, in 1916, while the country was deep in war with Germany. His primary concerns

15 In particuliar with the publication of the principal texts of Kondratieff in their entirety. translated into French from the Russian originals by Louis Fontvieille, *Les grands cycles de la conjoncture*, Economica, Paris, 1992, LIII-556 and index.

in this post were provisioning and the agrarian question. This was his position when the Russian Revolution broke out in 1917.

He then worked on the various undertakings of the *Commission on Agrarian Reform under the Central Agrarian Committee* and published a work on '*The Agrarian Question*' in which he set out his views. A member of the Socialist Revolutionary party, he was in favour of an orderly agrarian reform, based on the one hand on assignment of the land to those who worked it, by elimination of private ownership of land, and on the other on the development of co-operatives made up of volunteers. While a member of the *Committee on Provisioning of the Soviet of Worker Deputies and the Provisional Committee of the State Duma*, he became vice-president of the *State Committee on Provisioning* before being appointed Vice-Minister of Supply on 5 October 1917, just a few days before the October Revolution. Because of his opinions on the agrarian question, Kondratieff did not immediately join the October Revolution, fearing as he did a policy which would lead Russia "*down the path of famine*[16]."

Moving to Moscow in 1918, he specialised in the study of agricultural markets, in particular the market for flax at the behest of the *Central Union of Flax Producers*. Rapidly becoming a member of the Agricultural Academy of Peter the Great (which was to become the Timiriazev Agricultural Academy), he then published numerous works on the subject, some of which concentrated on analysing the mechanisms for adjustment of the ratio between agricultural prices and the quantities of agricultural goods produced[17]. It is thus not surprising that Kondratieff should develop research on the analysis of business cycles. He did so in two different posts:

- The first was the Institute for *Business Cycles* of the Ministry of Finance which he headed from the time it was established in 1920. This was to offer him the possibility of developing theoretical works on business cycles and on forecasting. He then surrounded himself with young specialists in varying domains (mathematics, statistics, economics, history of science) who – for example the young mathematician E. E. Slutsky – would subsequently become famous. He created two journals under this Institute[18], in which his preliminary researches on long-term movements[19] were to appear.
- The second was the *Department of Statistics and Agricultural Economics* at the Ministry of Land which, starting in 1923, was directly associated with the preparation of the forecast and plan for agricultural and forest development[20]. In this connection, in 1924 he travelled to various Western countries, including the United States, Canada, Britain and Germany to study the organisation of their agricultural production, development trends in their production, the intervention mechanisms and more generally the situation on the world market for agricultural products. For him, the purpose was to prepare the tools for planning and controlling agriculture[21].

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16 He published a first critical document on the policy of the Bolsheviks under this title, then a second one under the title *The Bolsheviks in Power: Socio-political Outcome of the October Coup d'Etat*.

17 Including: *The Production and Marketing of Oilseeds in the Interest of the Peasant Economy* (1921), *The Market for Cereals and the Regulation of it during the War and the Revolution* (1922), *The World Cereal Market and the Prospects for Our Cereal Exports* (1923).

18 *The Economitcheski Buleten Konjunkturnovo Instituta (Economic Bulletin of the Institute for Business Cycles)* which published statistical information on the Soviet State and on the world economy and a theoretical journal *Voprosy Conjunktury (Problems of Business Cycles)* for theoretical research.

19 'The World Economy and its Business Cycles Before and After the War' (1922), 'Discussion of Questions On the World Economy and Crisis' (1923) and 'On the Concepts of the Static, the Dynamic and the Business Cycle in Economics' (1924).

20 *The Bases of the Forecast and Plan for Development of the Agricultural Economy* (1924) and *Prospects for the Development of Agriculture* (1924).

21 This was to result in two publications: On the Problems of Forecasting and Plan and Forecast.

The synthesis of all of these different undertakings would subsequently feed into his thoughts on long-term economic dynamics.

In 1925, at a seminar at the *Economics Institute of the Association of Social Sciences Research Institutes* he presented an initial version of his thinking under the title '*The Large Cycles Within Business*.' The confrontation between himself and his opponents, in the forefront of whom we find Oparin, Bogdanov and Falkner, would cause him to become more stringent with his methods of observation and his general approach to the issues.

In 1928, he published two important works. One, picking up the title of his first paper ('*The Large Cycles Within Business*') set out to demonstrate the existence of long-term movements, and the other ('*The Dynamics of Agricultural and Industrial Prices*') sought to interpret them.

In terms of economic policy, this research led Kondratieff to adopt the idea that agriculture should be used as a basis from which to develop light industry, with the first step being to make the producers have an interest in the results of their work, through the action of the market. The co-operative form of development of agriculture then seemed to him the best way, socially and economically, of obtaining this result. The maintenance of a domestic market, by keeping a link with the world market, would promote the spread of technical or organisational innovations from which industry would benefit. It is evident that these views, compatible with the New Economic Policy, were incompatible with the priority which was to be given, starting at the end of 1927, to heavy industry and to the collectivisation of agriculture.

It was because of his involvement in the conduct of agricultural policy between 1920 and 1928 that Kondratieff would be dismissed from his various directorial posi-tions, and then arrested in July 1930, not because of his theories on large cycles. Sen-tenced to 8 years in prison, he was interned from February 1932 in the Suzdal "political isolation facility." At the end of a second trial in 1938, he was sentenced once more and shot at the age of 46.

1.2 Through all of this time, Kondratieff had built up a dense and closely-argues body of work. Until very recently, it has been difficult to know anything about it, from the few work of his that had reached us. But, even when they were translated, several problems, some of them cumulative, distorted the knowledge of Kondratieff's thinking.

<u>The first problem</u> is the language of the translation. For example, his approach to the long movements is generally considered as being very mechanistic. However, in his 1926 work '*Problems of Forecasting*[22],' he develops a probabilistic approach to business cycles on the basis of a consideration of the relations of causality and determinism in economics. While it was soon translated into German (although only partially), this article was to remain completely unknown and would almost never be cited by the long cycle specialists, doubtless because it was never translated into English. Only a few authors with an interest in the problem of planning would sometimes make reference to it[23].

<u>The second problem</u> is the quality of the translation. Kondratieff uses the term "*wave-like movements*" when he refers to the actual phenomenon itself and "*cycles*" when he is seeking the correct concept. The translations into German (*Wellen or wellen-förmige Bewegungen*) and French (*vagues ou mouvements en forme de vagues*) are correct.

22 In: *Voprosy konjunktury*, volume II, pp. 1-42. French translation in extenso from the Russian: 'Problèmes de prévision,' in: *Les grands cycles de la conjoncture*, Fontvieille L. ed., Economica, Paris, 1992, pp. 47-104

23 For example, Carr and Davies, *Foundations of a Planned Economy*, 1926-1929, Penguin, 1969.

What is less correct is the descriptor associated with these waves or cycles. It has become customary since the translation into German[24] of his works on the "long cycles," and has unfortunately remained so until the 1984 translation[25], to talk of long [*lang*] cycles (or waves), no doubt in order to stress the contrast with the short cycles, or by reference to the long term as the horizon of economic analysis, as Alfred Marshall was to develop this.

But, the term deliberately used by Kondratieff was "*large cycles*." For him the important thing was to indicate that the process repeats over time (hence his choice of the word "cycle"), but also that it leads to an overturning of all the economic conditions (hence the adjective "large").

We rarely find any trace of this intention of Kondratieff's in the Western literature, although this term was to be used by the German translator of the 1928 article on the '*Dynamics of Agricultural and Industrial Prices*'[26] to characterise the fluctuations of price movements ("*die großen zyklischen Schwankungen des Preisniveaus*"). We also find a trace of this term in the title of a German article critical[27] of Kondratieff's positions. In the French sphere, Verley[28] also used this term in a new translation of some extracts from Kondratieff's works.

<u>The third problem</u> is that of the choice of the texts for translation. There are several versions of the article devoted to the analysis of the "large cycles within business." The most complete dates from 1928, because Kondratieff integrated new data to refute the monetarist positions of his opponents, which allowed him to bulk up the theoretical framework of his concept of the large cycles. However, this document, the most interesting of all, has never been translated, although a participant in the various discussions did make a summary of it in a German journal[29].

The only text which has been available for a long time corresponds in fact to a preliminary version which restricts itself to studying the form of the wave-like movement and to recording the regular characteristics of each period. In this part devoted simply to a record of the research, Kondratieff also does not speak of phases, as Simiand was to do later, but of "long periods of increase or decrease of the business cycles" or alternatively of "periods of rising wave and of falling wave." These interim considerations cannot be taken as theoretical explanations of the phenomenon revealed, which he was not to develop until the final version.

This "interim proposal" was to be published in German in Schumpeter's and Lederer's journal[30] and it was on the basis of this version that it would be retranslated into English for publication in 1935 by the *Review of Economic Statistics*[31], but in an abbreviated version[32] which no longer contained the presentation of the

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24 Kondratieff N.D., 'Die langen Wellen der Konjunktur,' in: *Archiv für Sozialwissenschaft und Sozialpolitik*, Tübingen, no. 56/3, 1926, pp. 573-609

25 Kondratieff N.D., *The Long Wave Cycles*, translation from the Russian by Guy Daniels of the 1926 version, Richardson and Snyder, distributed by E.P. Dutton, 1984, New York.

26 Kondratieff N.D., 'Die Preisdynamik der industriellen and landwirtschaftlichen Waren (Zum Problem der relativen Dynamik und Konjunktur),' in: *Archiv für Sozialwissenschaft und Sozialpolitik*, Tübingen, no. 60/1 (August), 1928, pp. 1-85.

27 Herzeinstein, 'Gibt es grosse Konjunkturzyklen?' in: *Unter dem Banner des Marxismus*, no. 1 and 2, 1929.

28 Verley P. [1987], 'Commentaire d'extraits d'articles de Kondratieff,' in: *Revue française d'économie*, no. II/4, Autumn, 1987, p. 186 and ff.

29 Pervouchine S.A. [1930], 'Account of the Arguments on the Large Cycles within Business at the Moscow Institute of Economics,' in: *Weltwirtschaftliches Archiv*, Gustav Fischer, Jena, 1930.

30 Kondratieff N.D., 'Die langen Wellen der Konjunktur,' in: *Archiv für Sozialwissenschaft und Sozialpolitik*, Tübingen, no. 56/3, 1926, pp. 573-609.

31 Kondratieff N.D. [1935], 'The Long Waves in Economic Life' (translated from the German by W.F. Stolper), in: *The Review of Economic Statistics*, no. 17/7, November, 1935, pp. 105-115.

32 The complete text appeared in *Review* in 1979 (volume II, no. 2, 1979, Spring, pp. 519-562).

method, the economic significance of the "*secular trend*" (points II-III), or the studies on changes in wages and in international trade (points VI-VII). In this text, Kondratieff presents some graphs corresponding to the 9-year moving averages only of the deviations of the gross data relative to the "theoretical series," which caused many authors to interpret these curves as the smoothing of the actual movement of the economic components studied[33].

<u>The fourth problem</u> is that certain parts of the texts chosen for translation were cut out. By hiding a part of the demonstrations, this results in a deformation of the the-ses defended by Kondratieff, or even in a reversal of them. This is particularly the case with his study 'On the Concepts of the Static, the Dynamic and the Business Cycle in Economics[34]' dating from 1924, the English translation[35] of which is much shorter. The object of this text was to put forward the concept of the business cycle, as a working idea which would make it possible to observe and explain the conditions under which the process of change of the components of economic life takes place. This formal research led the author to redefine the notions of static and dynamic analyses, to break down the economic evolutions into their reversible and their irreversible aspects, and to differenti-ate the general business cycle from any particular simple business cycle, and from the differential business cycle. From that point, he deduced general principles for the use of concrete methods of observation, and in particular statistical methods. In parallel with his demonstration, he systematically sought to relate his own position to that of the great theoretical schools (from the physiocrats to the Marxians, via the classical theo-rists and the marginalists), or to the methods used by certain authors to analyse the business cycle (Sombart, Wagner, Schumpeter, Marshall, and so on). He even introduced a very modern discussion on exchanges of concepts between sciences, with regard to the use of the concepts of reversibility and irreversibility.

From all that, very little remains in the article published in the American journal: all of the references to business cycles and to the discussions have been systematically erased, with even the phrase "business cycle" being dropped from the title[36]. Thereby, all of the analysis of the irreversibility of the dynamic movement has disappeared, and as a result P. Norel, for example, can maintain nowadays that Kondratieff "*considered the time of the long cycle to be reversible*," while Norel holds the opposite view, namely "*the non-repetitive character of the long cycle, never returning to the identical*[37]." A further effect can be noted with Gattei: mistakenly, he thought that Kondratieff was not to develop his thoughts on the "*relative dynamic business cycle*" until '*The Dynamics of Agricultural and Industrial Prices'* in 1928, when the criticisms of his paper on the large cycles struck home, forcing him to respond to them by introducing some supplementary ideas[38]. The reality is different.

1.3 From the point of view of the history of economic thought and of the relationship between Kondratieff and Schumpeter, this problem of translation is important. In the paper '*On the Concepts of the Static, the Dynamic and the Business Cycle in Economics*,' he justified his preference for the concept of the business cycle, which

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33 We may quote H. Guitton (*Les fluctuations économiques*, Sirey, Paris 1951, p 160) and more recently N.H. Mager (*The Kondratieff Waves*, Praeger Publishers, New York, 1987, p 29).

34 Kondratieff N.D., « 'On the Concepts of the Static, the Dynamic and the Business Cycle in Economics,' in: Sotsialistitcheskoie Khoziaistvo, volume 2, no. 4, 1924, pp. 349-372.

35 Kondratieff N.D., 'The Static and the Dynamic View of Economics,' in: *Quarterly Journal of Economics*, no. 39/2, 1925, pp. 575-583.

36 A persuasive example can be supplied with the following phrase from the 1925 article "having defined the static and dynamic points of view, we have to characterise the various forms of dynamic processes," whereas in the original, it was "having defined the static and dynamic points of view, **and explained how we differ with respect to other attempts of this type**, we have to, **for the following definition, that of the business cycle**, characterise the various forms of dynamic processes" (words in bold are deleted from the American version).

37 Norel P. [1991], 'Cycles longs Kondratieff et crises: une approche épistémologique,' in: *Économies et Sociétés, Histoire quantitative de l'économie française*, AF no. 16, February, 1991, p 174.

38 Gattei G., 'Introduction to N. D. Kondratieff,' in: I *cicli economici maggiori*, Cappelli, Bologna, 1981, p 13.

led him to criticise the wrong trail which Schumpeter was following in analysing the stationary economy in order to understand economic development[39].

The perception of the unity and continuity in time of the theoretical concerns of Kondratieff was thus completely distorted by the conditions under which his works were distributed outside the Soviet Union, with the latter doing nothing to improve the situation by hiding his works under a bushel for almost 60 years.

Kondratieff's theoretical project was a mature one from the point of view of its object and its methodology. The theory of the large cycles is far from being just a commentary on statistical curves. On the contrary, production of them is one component among others. Their function was to corroborate or refute pre-existing hypotheses. This work of investigation was not incomplete, because it was only provisional, nothing more than one stage in a wider project. Unfortunately, Kondratieff was not able to continue his research, and what we have is a body of work which, while promising, is uncompleted.

In order to have a picture of Kondratieff's difficult intellectual position at the end of the Twenties, when he was trying to defend his hypotheses concerning the "*probable existence*" of large cycles in the long business cycles of the economy, we could use the same words as those of Keynes at the end of the preface of the first edition of his *General Theory*: "The difficulty lies, not in the new ideas, but in escaping from the old ones, which ramify into every corner of our minds."

Indeed, the nature of the criticisms directed at his general method of approaching the long movement reveals clearly the lack of understanding of the theoretical concept underlying the theory of the large cycles and the difficulty of conceiving of development in a new way. This idea, apparently extremely simple, that the rhythm of the irreversible movement of development is rendered by the reversible movement of its structural components calls into question all the benchmarks of the existing theories, and construes everything quite differently. Indeed, this idea which combines differential analysis and dynamic analysis of the business cycle veers off towards a systemic approach to the long movement, the regulator of which might be the changes in the relative prices as between industry and agriculture. However, it would be erroneous, in seeking to renew the meth-odology for approaching the large cycles, to assign unalloyed virtue to Kondratieff's analysis of them, or to present it as a precursor of the theory of regulation. The explanation supplied suffers, in fact, from an internal flaw of logic relating to the role attributed to agriculture and from a complete lack of any theory of distribution, which in turn makes the mechanism of distribution of the transfers of values totally obscure.

2. Kondratieff's methodological framework

Kondratieff has never been truly recognised as a fully-fledged economist, as can be seen from the absence in economic literature, starting right from Schumpeter in his *Business Cycles*, of any attempt to make a general presentation of the goals of his research or the issues related to it.

On the other hand, the opinions expressed on his body of work are numerous, although they are as divergent as they are emotional: Marjolin dismissed Kondratieff as a "vulgar materialist," the better to be able to ignore him subsequently, whereas Imbert incensed him by "parking" him among the Marxian theoreticians. While the former certainly had not read him, the latter did not attempt to justify his classification by showing how a link is created, for example, between the large cycles and the law of the tendential decrease in the average rate of profit.

In fact, the implicit criterion by which Kondratieff is judged always lies, including in the multiple intermediate variants between those two extreme positions, in the assessment of the validity of the method he used in order to demonstrate the existence of the "large cycles."

39 This had to do with the following works of Schumpeter: *Das Wesen und der Hauptinhalt der theoretischen Nationalökonomie*, Duncker und Humbolt, Leipzig, 1908 and *Theorie der wirt-schaftlichen Entwicklung*, Duncker und Humbolt, Leipzig, 1912.

As a consequence of this focus of the criticism on the mode in which the statistical data were processed, the judgment by the different authors on Kondratieff's approach almost always starts out from the result, namely the statistico-empirical demonstration of the existence of the large cycles, irrespective of any examination of the prior theoretical path followed to reach that point. From this point of view it is astonishing to note that between the article by Garvy in 1943[40] and the work of Grangeas[41] in 1991, the order in which Kondratieff's work is presented remains the same: first "the method" (reduced to the statistical processing), then "the reach of the results," and finally "the criticism[42]."

The poor way in which Kondratieff's body of work has been distributed may largely explain this difficulty in reconstituting the genesis of his thought in order then to give the proper weight to the importance in it of his instruments of statistical verification. But that cannot explain the whole situation.

This approach to the work of Kondratieff is also symptomatic of the methods generally used by the theoreticians described as "long cycle." For a very long time, right up to today, the latter, setting out from statistical series representative of the result of economic activity (price, production and consumption levels, etc.) thought a priori to reflect the overall movement of the economy, have sought a visual – and thus supposedly objective – trace of the wave-shaped movement[43]. A chaining together of the explanatory causes was then deduced from that.

Kondratieff's investigative method is thus reduced to no more than the statistical tool selected (the "trend deviation") plus the choice of the mode of pre-processing of the series selected (conversion to constant prices from gold prices, normalisation). This definition is of course much too restrictive. It does not answer one question which is, however, essential: why and how did Kondratieff come to be using these tools of empirical verification?

Of course, it could be considered that he was no more than a skilful manipulator[44]. Some people in the Twenties, in the arguments caused by his work, were not far away from thinking that. But this hypothesis has to be rejected: it matches neither the quality of exposition and synthesis of his articles (including in their translated versions), nor the responsibilities which were entrusted to him after the Revolution of October 1917.

Rather, it was the whole prior theoretical path which he followed to understand the causes of price movements relating to agricultural production that was to cause Kondratieff to define the "large cycles" as a relevant and scientifically-based subject for analysis of the conditions of development of the market economy.

It can consequently be maintained that the theory of the large cycles, initially understood as a theory of a rhythmical economic development, is pre-existent in Kondratieff to the statistical demonstration of it[45].

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40 Garvy G., 'Kondratieff's Theory of Long Cycles,' in: *The Review of Economic Statistics*, vol. 25, no. 4, November, 1943, pp. 203-220

41 Grangeas G., *Croissance, cycles longs et répartition*, Economica, Paris, 1991, 326 pages.

42 Note must nevertheless be taken of a major difference between the two authors, even if this does not have an impact on the argument put forward: the first author refutes the existence of the large cycles, the second does not.

43 Imbert, for example starts his work with a very long first part devoted to "the statistical analysis of the long-term movement," before going on to "the statement of the theories" and "the nature and the causes" of this long-term movement (*Des mouvements de longue durée Kondratieff*, Bibliothèque de la Faculté de Droit et des Sciences Économiques d'Aix-en-Provence, La Pensée Universitaire, 1959).

44 Or even a usurper of a "discovery" made by others, if one accepts the arguments of Baran and Sweezy, when they write on the subject of long cycles "called by Schumpeter 'Kondratieff cycle(s)' to honour the Russian economist who first claimed to have discovered a cycle about fifty years long and relating to the history of capitalism in the 19th century and the beginning of the 20th," *Le capitalisme monopoliste*, François Maspéro, Paris, 1970, p. 209, note 12.

45 Kondratieff is also explicit in his theses on large cycles: "In order to establish whether these large cycles really did exist, we studied the statistical data..." ('Les grands cycles de la conjoncture,' in: Les grands cycles de la conjoncture, Fontvieille L. ed., Economica, Paris, 1992, pp. 165 [noted below in Kondratieff 1992d]).

By contrast to what is generally held by his critics, he did not develop an induc-tive method. On the contrary, he differentiated himself radically from the historical German school, and, on this specific point, from Tugan-Baranovsky. It was his conception of capitalist economic development that led him to seek the tools which would be the most suited to empirical investigation. The extreme virulence of the arguments caused by the publication of his works in the Twenties can be explained in part, leaving aside the particular socio-political context of the Soviet Union, by the innovative way in which he raised and resolved the problem of long-term movements.

In consequence, the analysis cannot be limited merely to the technical aspects of the methods, but has to cover also Kondratieff's general methodology within which the latter are produced and incorporated.

2.1 Dynamic analysis of the business cycle and the hypothesis of large cycles

Kondratieff sought throughout his work to make his scientific approach explicit by laying down a number of methodological principles and by defining stages in his researches. We find these concerns reflected in all his writings. During the arguments caused by his various publications (on business cycles in 1922, on the definition of the static relative to the dynamic and of course on the large cycles) he always states the methodological framework which he has set for himself. Very often, he admonishes his opponents on questions of method and a lack of rigour in their demonstrations. He also always sought to locate his position relative to the major theories and with respect to the work of other international institutes working on the same subject (for example, he placed the concerns of the *Institute of Business Cycles*, of which he was the director, at the same level as those of the Harvard Committee)[46].

2.1.1 Kondratieff's theoretical objective was to understand the socio-economic reality in its long movement of transformation. This is by definition "*fluctuating, varied and complex*," making it "*mobile and impossible to grasp*." The whole challenge of a scientific method is to break it down into simpler and more homogeneous components, allowing each of its relevant aspects to be studied separately and in depth. Kondratieff stressed that doing so results in a tendency to shift the question towards that of delimiting particular fields of research and creating tools suitable for investigation into the real.

As a result, the point of view that one chooses to simplify the real is not a matter of indifference and forms part of the scientific concept. Indeed, since the theoretical construct is no more than a reconstitution of reality, it becomes dependent upon the point or points of view that one has favoured in order to bring about the simplification of the objet of the study. These points of view become determining because the concepts used will flow from them, while the essential criterion of their relevance has to be their capacity, on the one hand, to simplify the empirical investigation into objects and into elementary and concrete relations favourable to particular studies, but also on the other to represent the complexity of the whole of which they are only one of the manifestations.

The quality of the point of view chosen will determine directly the quality of our capacity to understand the real movement, an understanding that has to be expressed in the form of a statement of laws. This approach led him to place as the horizon of scientific work, and thus as the horizon of his own work, the production of laws of behaviour or alternatively of empirical regularities. The latter can only be obtained at the end of a necessary process of comparison and contrast between hypotheses (arising out of the particular point of view for approaching the problem to be studied) and empirical investigations on elementary phenomena correctly

46 Kondratieff N.D., 'Sur les concepts de statique, de dynamique et de conjoncture en économie,' in: *Les grands cycles de la conjoncture*, Fontvieille L., ed., Economica, Paris, 1992, p. 6 [noted below in Kondratieff 1992b].

selected. This comparison and contrast converges into the current practice of the experimentation whose purpose is to reproduce the behaviour that is imagined to exist between different elements at a given moment and under defined conditions.

Consequently, a law results primarily from our capacity to reproduce a given phenomenon in a simplified manner. But it must never be confused with the phenomenon itself which, in its concrete manifestation, remains dependent on other elements. In order to illustrate this caveat, Kondratieff gave the following example: it is pointless to "*refer to the law of gravity (to) explain why yesterday a stone fell off the roof of the house which you were just passing*[47]" .

This presentation is very much marked by the practice of the exact sciences as is shown by its large number of references. He is thus then brought to envisage the particular application of these principles in the "*sciences of culture*." In *Problems of Forecasting*, he examined in detail the reasons for the difficulties which they encounter in bringing this scientific procedure to its conclusion, namely to the performance of the experiment. The reason lies in the irreversible character of the socio-economic phenomena. This situation means that all concrete phenomena are unique and the result of a very large number of causes. Kondratieff notes that the situation is the same for the natural sciences, but to a lesser degree: "*The unique character of the phenomena of nature is less striking than in the case of culture. This offers to science the possibility of bringing closer, and rendering more exact, the determination of the causal relationship between the phenomena of nature than*" between those of culture.

However, with regard to the typical features as compared between the phenomena of nature and culture, the difference is only quantitative, not qualitative. Given that it is impossible to master the totality of the causal interlinkages[48], this difference has simply to impact the mode of experimentation, not the actual principle of the experiment.

Experimentation in the natural sciences finds it feasible to reproduce the inter-linking of the causes because they are very few in number and because the physical components are stable over time. From that phenomenon arises the regularity of the behaviours. Consequently, experimentation in the sciences of culture will have to set out from "*the regularity of the progression of events*" which he describes further with: "*By the term 'regularity, we understand the uniformity with which events occur*." Generalisation will consist then in revealing the essence of a <u>regular phenomenon</u>.

But how is one to proceed in order to extract from the concrete movement the constant and regular components that allow independent generalisations concerning their historical context? The answer is that a causal relationship supplies only the *conditions necessary* for the actual performance of the event. Those are what must be sought. Consequently, experimentation entails the study of "*the notion of regularity (which) is grounded positively in the real existence of uniformity and the repetitivity in the world that surrounds us*." Kondratieff stated that one can rely on the notion of uniformity because "*relative to the events observed in their totality*" the effects of the actions of this or that component, even if they are determined and necessary, will be "*relatively random.*" One could therefore rely on the regularity observed as an approximate expression of the real movement.

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47 Kondratieff N.D., 'Questions controversées d'économie mondiale et de crise,' in: *Les grands cycles de la conjoncture*, Fontvieille L., ed., Economica, Paris, 1992, p. 510 [noted below in Kondratieff 1992f].

48 Kondratieff states "unless one were the almighty reason," which is a barely veiled allusion to the methods of the Gosplan ('Problèmes de prévision,' in: *Les grands cycles de la conjoncture*, Fontvieille L. ed., Economica, Paris, 1992, pp. 59 [noted below in Kondratieff 1992c]).

2.1.2 Kondratieff had also studied under Tugan-Baranovsky. He was familiar with the analysis of crises and of development, and was well acquainted with Tugan-Baranovsky's analysis of industrial crises (sectoral disproportionalities and available savings). But he also knew well the latter's dissatisfaction with the explanation of the nature of the evolutions which occurred during the 19th century: "*Contemporary observers are not, normally, capable of distinguishing periodic fluctuations from the more profound changes in the sphere of industry*[49]."

Kondratieff took up this subject. He did so firstly because if was to specialise in the problems of development of agriculture, he had to determine the constraints on its development and, to some extent, the nature of the links binding agriculture to industry. In such a context, the theory of industrial cycles is of course far from adequate. What was demanded was to determine, taking into account the regularities in the relations between industry and agriculture, the form that development of agriculture in the Soviet Union should take, and the supporting policies (price policies, integration into the world market, types of crops, types of investments) that would be required.

These concerns (evolution of agriculture and of its relations with industry) decided him to define the object of his research as being the search for the economic dynamics expressing the combination of two different movements found in all economic variables: an irreversible evolution and a reversible behaviour, these being found whatever time horizon is selected.

Starting from the finding that certain variables mainly experience a movement with a given orientation but with changes in their rate, and that others initially manifest themselves in the form of cyclical oscillations with a change of level, Kondratieff deduced that irreversible and reversible processes are present in all variables. Methodologically, he deduced that the study of the reversible movements could be separated from that of the irreversible movements, even if they are linked in the course of the general process of development. He indicated that he was taking "reversible processes as the object of study." Consequently, "the study of the dynamics of these reversible processes is the study of the shift from one cycle to another[50]."

The analysis of the business cycle will be that of the evolution of this reversible movement, taking into account the evolution of the relations between sectors or branches (the differential business cycle). The hypothesis put forward is therefore that the reversibility observed translates an internal movement which leads periodically to the return of a given particular business cycle. For Kondratieff, in consequence, a cyclical movement simply translates the regularity of an economic process which causes certain particular business cycles to succeed one another periodically in a given order, each of them reproducing the causes for the appearance of the next one.

On the basis of this hypothesis and of remarks by Tugan-Baranovsky and other authors, or of his own findings, "*we find an empirical regularity in the changes, not only of the small cycles, but also of the large business cycles of the capitalist economy*." From there, Kondratieff reached the conclusion that there exist regular and repeated movements in the process of development.

Starting in 1922, he described them and defined them as the specific object of his study: "*The only phenomena that concern us here as the direct object of our research are the reversible processes of the dynamics of the business cycles. We make a distinction between small cycles of 7-10 years and large cycles of 40-50 years, those which we have been trying to track, starting from the end of the 18th century*." At the end of his empirical experimentation, he would be able to state the average length of those large cycles.

49 Tugan-Baranovsky, 1913, op. cit, p. 33.
50 Kondratieff, 1992f, p. 497.

2.2 Statistical materials and methods of observation

Kondratieff adopted as his own the principle of Tugan-Baranovsky that "*no economic theory can be considered as being absolutely demonstrated until it has been verified by the facts*[51]." Consequently, he sought to set up statistical series which would allow an empirical approach to the economic elements that his prior analysis of the principles of the dynamic business cycle had revealed as being relevant. The purpose of the empirical verification which he undertook was not to establish laws on the overall movement of capitalist society. Quite the contrary, "*at the current stage*" of his work, he was interested only in analysing the principal elements of the capitalist economy in the evolution of their relative business cycle, and more specifically, a verification of his hypotheses[52].

In order to do that, he used the techniques of mathematical statistics in order to be able to separate *secular trend* and *large cycles*. This method brought him both the hostility of the other economists and notoriety, with the convergence of those two attitudes having the effect of that he reduced his methodology to making use only of a few specific techniques.

2.2.1 Initially, he sought to set up a sufficiently large range of all the variables useful for verifying that there really are long waves in the business cycle, without however attempting to be exhaustive. Many elements supplied by Kondratieff in the course of the arguments reveal this approach conclusively. Thus, when Oparin complained about his making use of English lead production although it was small, he replied that the question was not to determine "*the relative importance of England in the world production of lead, but to find out whether there are fluctuations in the production of lead, and if so, what they are*." Thus the purpose of the series was not to reconstruct the real movement. On the other hand when Kondratieff's objective was to understand the real movement of evolution in the relative prices between industry and agriculture, he would collect all of the data which would allow him to construct his price indices.

The structure of the sampling of the series is defined by reference to the different types of behaviour which economic variables may present in their development, as he had described them in *On the Concepts of the Static, the Dynamic and the Business Cycle in Economics*. He identified two main groups on the basis of their being somewhat more reversible (elements expressed in value such as prices of goods) or somewhat more irreversible (elements expressed in volume: production and consumption) as well as mixed groups corresponding to the various intermediate situations (interest rates, wages, bank deposits, balances of external trade). Under the constraint of covering the whole of the period of development of capitalism for each of these groups, the selection of the countries had to be limited (to France, to England and also to the United States) as did that of the series. Only the most significant were presented in his texts in support of his demonstrations.

In order to make possible a comparison over time, Kondratieff normalised the data on the one hand by converting the values into gold prices (always going through the dollar equivalent) and on the other hand by establishing ratios on the basis of the population. The "empirical series" to which reference is made in the texts correspond to data which have undergone this sequence of operations. The principle of normalisation did not raise any problems in Kondratieff's opponents: they simply objected to the methods used for it. Oparin, for example, considered it useless to divide by the population, in the case of the volume series. More fundamentally, the monetarists considered it necessary to divide the value series by the price index and to amplify the list of the series by adding the data on the evolution of the quantity of gold in circulation. Kondratieff natu-

[51] Tugan-Baranovsky, 1913, op. cit., p 222.

[52] "In my work, by taking empirical series, I was asking myself if it was possible and relevant to process them in such a way as to learn their structure and to obtain such or such a response, affirmative or negative, on the existence of the long fluctuations in these series. I recognised the necessity of such processing." [Kondratieff 1992d, p 265].

rally refused this procedure which in the final analysis equated to interpreting *a priori* the results of the investigation being undertaken.

2.2.2 It is not enough to have set up a sample of statistical series representative of the different types of behaviour of the economic variables over the long period, because the latter remain "complex entities." Kondratieff was confronted with the problem of breaking down each of them into reversible movements and irreversible movements. This then raised the question of statistical method. In order to obtain this result, he put forward the suggestion that he should separate the general trend (also called the "secu-lar trend") from "*the acceleration of this basic trend*."

In his first works, Kondratieff used the traditional method of smoothing, which entailed using a moving average to get rid of short fluctuations. But this method was not suitable for his objective, not giving him the breakdown between general trend and cyclical movements that he had anticipated. He thus sought a different method which would allow him to determine the secular trend on the basis of the processing of all of the data in a series, and by subtraction to reveal the reversible aspect of the movement. The objective was subsequently to study the form of the curve obtained from the annual deviations from the general trend of the series. The whole question, of course, was to find the technique for extracting the trend. The visit which Kondratieff made to the United States in 1924 was an opportunity for him to obtain a deeper knowledge of the work which Harvard University had been pursuing since the 1910s on the same subject under the direction of Warren M. Persons, involving the creation of "*economic barometers*" able to predict the evolution of the business cycle.

In order to calculate the trend of a series, Persons used the method known as "least squares," which means that the series is adjusted by a linear function the type and equation of which are determined in the light of its graphic representation[53]. Next, the data are recalculated over the whole of the reference period in order to obtain a "theoretical series." By calculating the deviations between the two series, one obtains the expected residual movement. The new series will be smoothed to eliminate random variations. The whole set of curves (4 in total) can then be transferred to a graph.

Kondratieff adopted the whole of this procedure known today as "*trend-deviation*" to process his own series[54]. A smoothed moving 9-year average calculated on the deviations – the nine years corresponding to the average length of the short cycles (7-11 years) – "*then reveals large cycles in the raw state*," but the work of calculating adjustments to all of the series was complicated, long and tiresome, and would take several years to complete. Thus, when Oparin in a carping criticism of his calculations was to reproach him with not updating his series with the most recent data, Kondratieff was to reply in exasperation "*that updating an empirical series always requires you to revise the theoretical series, and (that) that is a colossal task*."

The trend deviation method was thus developed primarily in a search for accuracy. It consists of comparing on the same graph the evolution of a certain number of variables, in such a way that observation of the regular repetition of shifts in the changes of direction of the different series make it possible to predict the changes in the business cycle[55]. The variables are represented not by the raw or theoretical data, but by the smoothed deviations. Kondratieff in his first reports on the large cycles adopted exactly the same presentation, consisting of superimposing several different series[56], with the aim of showing the existence of synchro-

53 Persons W. M., 'Construction of a Business Barometer Based upon Annual Data,' in: *American Economic Review*, vol. VI, no. 4, 1916, pp. 739-769.

54 Imbert therefore maintains, wrongly, that "the method of the 'trend-deviation' had been discovered and applied with all the rigour desirable by N.D. Kondratieff," op. cit., p. 90.

55 Kondratieff gave a detailed presentation on these techniques in 'Problems of Forecasting' [Kondratieff 1992b].

nous movements for series of the same nature. This procedure was roundly criticised by Oparin, and rightly so. In the subse-quent versions, Kondratieff took care to present his diagrams in three distinct parts and to individualise them in order to avoid any confusion between the real movement and anything which was only the result of processing of the data.

2.2.3 But the fact that the origin of the method was related to the problem of forecasting led numerous authors to seek to extrapolate the curves of the "large cycles." Kondratieff always refused to concede any value whatsoever to any interpretation of the continuation of the trends, because those particular curves were not and could not be those of the real, concrete and historical movement of the long business cycle. The evolution of the business cycle could only be probable, without any certainty as to when and how fast the trend would change direction. In another part of his response to Oparin, he stated clearly that "*for anyone who knows a little about the least squares method, it has to be evident that, without sufficient bases, it is impossible simply to prolong a theoretical curve beyond the limits of the empirical series*."

In fact, the question raised by this temptation to prolong the trends revealed by the processing is that of their nature and their economic meaning. When Oparin heaped up the "technical criticisms" on the method itself and on the particular use which Kondratieff made of it, it was primarily in order to deny that there was any economic meaning in the theoretical curve, as an expression of the secular trend, and consequently to demonstrate that any deduction from it as to revelation of the existence of "large cycles" was on very shaky ground. Indeed, the heart of the verification of the actual presence of large cycles in the different types of curves is based upon the necessity of eliminating the secular trend. Is it possible for the result of adjustment by the least squares method to achieve this result?

At all times, Kondratieff's reply was precise: "*Firstly, it is true that we do not yet have a method for determining perfectly precisely the real trend of the secular movement. (...) Secondly, with a degree of approximation, we can nevertheless grasp the trend of a given empirical curve*." The theoretical series is not the representation of the trend, but it is assumed that it "*reflects fairly exactly the trend of the given empirical series*." The various criticisms are then interpreted as an expression of the limitations specific to the method, of which Kondratieff is perfectly aware through his regular reading of the publications of the Harvard Committee, and not as the refutation of the procedure itself for revealing the large cycles. Nevertheless the problem was a real one, which caused him in his role as director of the Institute for Business Cycles to start a programme of specific research on it. It is in this context that Slutsky developed his thesis work, the results of which would appear in a famous article in the American journal *Econometrica*, in 1937[57].

The most important limitation of this method stems from the fact that by definition the "*movement is not independent of the period envisaged*[58]" as noted by Lacombe with whose work Kondratieff was familiar. Virtually all of the criticisms formulated by Oparin do not do anything more than give expression to this limitation from its differing points of view. Kondratieff would attach only a little importance to the actual thrust of these comments on his general approach, because their practical consequences are so minor "*that they have absolutely no impact on the conclusions concerning the existence of large cycles*." The first criticism refers to the orientation of the curves at their two ends. It is true, in fact, that depending on the degree selected for the adjustment function, the form alters at the ends. A first element of response by Kondratieff was to estimate

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56 His 1926 article ('Die langen Wellen der Konjunktur,' op. cit.) repeats this presentation.

57 Slutsky E., 'The Summation of Random Causes as the Source of Cyclic Processes,' in: *Econometrica*, Vol 5, no. 2, April, 1937, pp. 105-146. A first version was published by the Institute for Business Cycles in 1927 in the journal *The Problems of Economic Conditions*, Vol 3, no. 1. In the same issue, we find an article by D. I. Oparin, 'The Method of Schematic Deviations Applied to the Dynamics of the Cotton Market.'

58 Lacombe E., *La prévision en matières de crises économiques*, Paris, Marcel Rivière, 1925, p. 58.

the real consequence of this inaccuracy on the economic analysis: the start corresponds to the period of development of the capitalist economy (end of the 18th and beginning of the 19th centuries) and is therefore in any event difficult to define; while the end of it represents an exceptionally unstable period (the World War and its consequences). In fact, and this is the second element of response, the alterations of the ends reflect the greater or less accuracy of the adjustment on the whole of the empirical data. What was important, therefore, was the general representation of the curve obtained. However, there is no objective rule for determining the best adjustment function. The question becomes a question as to the choice of the function. Oparin maintained that this choice would make it possible to create cycles "*artificially*." Kondratieff was cut to the quick by this criticism which called into question the rigour of his scientific approach. He recalled first of all that this statistical processing was only one stage of his project intended to verify on empirical data whether certain hypotheses were correct. He then stated that the risk was not so much that of making cycles appear as of making them disappear, depending on the nature of the function selected to adjust the series. The precise choice of the characteristics of the function depended solely on the goals in view. Since he was looking for large cycles, he had to reject particular functions (for example, those giving sinusoids) or high-degree functions which would absorb the phenomenon. What he had done that was arbitrary, therefore, became no more than the choice of the most suitable tool to resolve the scientific problem raised[59].

This first series of criticisms was then joined by a second concerning the consequences that extending the empirical series would have on the determination of the adjustment functions. In fact, for any one function of a given type, this takes the form of a marginal modification in the parameters, and thus partially in the form of the curve. Major modifications, if any, should occur only over the long term, raising not the problem of the calculation method but that of the empirical movement causing them. Finally, Oparin did not understand why Kondratieff used very different functions to adjust empirical series of the same nature (the French pension fund and the English Consol). This criticism is based on the hypothesis that for a given phenomenon there has to be a corresponding secular trend, and thus a type of adjustment also. In consequence, if we have two types of adjustment that means that the least squares method is not appropriate to the object of the problem. We come back to the starting point on the meaning of the adjustment. Kondratieff rejects this syllogism and asks the question the other way round: why, "*despite the unity of the capital market and the links between England and France, was the empirical curve of the French pension fund of a different shape from that of the Consol*?"

Finally, the last series of criticisms refers to the number of complete cycles which Kondratieff had been able to demonstrate. The argument was that the number of cycles obtained (2 to 2½) was insufficient to confirm the certain existence of a clear wave-like movement. Kondratieff had no difficulty in conceding that its existence was no more than probable. He was nevertheless satisfied with the result because he had succeeded in finding cycles throughout his sample for all the principal series of the same nature and with virtually the same timing. He **felt himself empowered to continue his research**, the more so as he detected a degree of ambiguity in his opponents. Some of them (including Oparin), despite the technical arguments put forward, were nevertheless ready to recognise the large cycles where their theoretical thinking could admit them, and to reject them for the volume series where the explanation originating in monetarist theory encountered a greater number of difficulties.

59 It can be remarked on this aspect of the question that the work of Persons covers only the period 1879-1915 and that only first-degree adjustment functions are used to reveal the medium-sized cycles. Lacombe (op. cit., p. 52) indicates that if he had used a longer reference period (1810-1920), he would have had to use a more complex function of the fourth or the fifth degree, and therefore a curve more clearly following the empirical data. However, Kondratieff for the same period took some functions of signficantly lower degree, and was right to do so, because he was not working on medium-sized cycles.

3. Interpretation of the large cycles

In Kondratieff's view, the object of using the *trend deviation* method was not to measure fluctuations, and thus to compare different real situations.

The point was to verify that the oscillations observed in the rates of growth of certain variables or in the absolute levels of others could be revealed in the form of a movement with a discontinuous orientation. That made it possible to determine the probable profile of the large cycles. By comparing and contrasting the profiles obtained with the empirical regularities defined elsewhere on qualitative variables of economic life (known under the term "empirical laws"), Kondratieff sought to make explicit the conditions of the cyclical process, by detecting the conditions necessary for a shift from one short business cycle to the other within the framework of a single cycle.

By hypothesis, he sought to take account of the endogenous conditions, in other words those internal to the economic activity. Synthesising those different concerns allowed him to define the draft of a theory of the large cycles.

The formulation of this theory drew vigorous criticism. We have referred to the dispute only where this was necessary to allow for the most complete presentation of Kondratieff's position.

3.1 Essential capital goods and degree of development of the productive forces

The "large cycles" characterised the conditions in which capitalist economic activity unfolds over time. The explanation for them, that Kondratieff was careful to present more as a hypothesis than as a completed theory[60], could be looked for in the internal laws of development of capitalist economic activity: "*The large cycles which we discover cannot be explained by random exogenous causes. The explanation for them must evidently be looked for in the particular features of the capitalist economic system*[61]."

Kondratieff then indicated that the basis in material terms of these "large cycles" would probably reside in the conditions of wearing-out and renewal of the "essential capital goods" and that the rhythm of the large cycles would reflect the rhythm of their process of growth. But the definition of them remains imprecise and ambiguous. We find in fact two different approaches to them. This vagueness attests to the difficulty encountered by Kondratieff in defining not so much the nature of the productive forces themselves from the material point of view as their degree of development from the social point of view.

3.1.1 The first approach consisted in dividing goods up into three categories, using this double criterion of the wearing-out time and the scale of the investment to be undertaken ("*the scale of the grouped investment*" as it is technically called). The essential capital goods fell into the third category, characterised as goods which "*function for several decades, and which require a great deal of time and enormous investments to produce*." Kondratieff drew up a list of them to clarify the idea (large buildings, the construction of major railway lines, canals, major civil engineering work, etc.) although he did indicate that the boundaries between categories were imprecise.

60 He stipulates, indeed, several times over: "it seems to us, in conclusion, that the hypothesis put forward, as a first attempt, gives an explanation..." and later "as an initial explanatory hypothesis, one may propose the following conception.." Kondratieff [1992c], p. 164 and 167

61 Unless indicated otherwise, the reference of the quotations in this part is Kondratieff, 1992d.

This approach is the exact counterpart of the theory of Alfred Marshall concerning the different types of equilibrium, and Kondratieff drew attention explicitly to the closeness of their theories. It is also consistent with his criticism of the crisis theories which he charges with analysing the concrete development of economic activity on the basis of an abstract scheme of sectoral distribution of production[62], which would be possible only if one "*possessed at a given moment a complete knowledge of the impact of all the causes and the disposition of the elements of reality*" as he indicated in 'Problems of Forecasting[63].' Kondratieff finished off this argument on the scientific method to be used for investigation into reality by adding a reference to the specific procedure of Marx himself who "*affirmed that the basis in material terms of the periodic crises which repeat every decade (...) is the wearing-out, replacement and extension of the mass of the tools of production*." We may observe that Kondratieff is here applying a significant shift to Marx's thinking, in deducing from it that the starting point of the analysis is the study of the time-related conditions of wearing-out and renewal of the capital goods. From the theoretical point of view, the challenge of this discrepancy between Marx's hypothesis and Kondratieff's interpretation is to analyse the conditions under which the social reproduction of the material and financial conditions of accumulation take place over a long period of time. The reference to the wearing-out time of the means of production is in itself insufficient to establish that he is methodologically a descendant of Marx.

We grasp very quickly the consequences of this point of view once we examine the statistical materials gathered and the way they were processed. We find virtually no statistical series having to do with the evolution of the annual productions of these essential capital goods (with the exception of railways), nor, obviously, any on the stock available and in use (in physical and/or value terms). This last question is of course an awkward one from the point of view of the sources. He had worked on the question a few years earlier of evaluating the capital in terms of stocks and shares invested: he stressed that "*recalculating the capital held in shares is not a simple operation*[64]," recreating it over a century was doubtless even less simple. He was subsequently never to bring this indicator up again.

It may be thought nevertheless that the problem is certainly not simply one of sources. For example, we may note that the calculations of productivity which Kondratieff undertakes in *The Dynamics of Agricultural and Industrial Prices* to demonstrate that changes in the price of gold should not be linked to the evolution of the quantity of gold in circulation but to that of productivity, are never referenced to the quantity of the capital goods available, nor to the amount of capital employed, nor to its structure. It is clear that he was seeking to bring in the apparent productivity of labour[65] only as an indicator of the evolution of the purchasing power of money and not, by way of this admittedly very imperfect result indicator, the evolution of the fluctuating conditions of the profitability and performance of economic activity.

Ultimately, this approach comes down to an additive approach to the essential capital goods viewed individually, although when grouped together they are considered as being motors of the mechanism of the large cycles. This, of course, stirred up a large number of objections.

62 In this connection, it may be remarked that while Kondratieff refers to all of the Marxian economists of the period (Hilferding, De Wolff, Parvus, Kautski, etc.), he never cites Rosa Luxemburg.

63 Kondratieff, 1992c, p. 59.

64 He proposed to calculate two series expressed at constant prices: the total volume of the capital actually invested (equal to the value of the stock of capital at the beginning of the year plus any increases of capital over the year) and the total market value of the capital in the form of shares (equal to the total value of the shares at the beginning of the year corrected by the average rate of the shares of that year, and increased by the total of the shares relating to the increases in capital for the year), Kondratieff, 1992f, p. 536 and ff.

65 Division of the physical volume of the production by the manpower employed, in indices to obtain the evolution of productivity, Kondratieff, 1992e, p. 407 and ff.

The list which one obtains by applying the two criteria of time and mass causes the essential capital goods to be defined as items of equipment which cannot be limited to traditional means of production, or in other words items of equipment which take part in the actual act of transformation of materials. It might be found surprising that the criticisms did not mention this aspect. In fact, as the majority of them, following Oparin and Falkner, denied the very existence of movements of long duration for the physical series, this question was not of any particular interest. It may be that they followed the line of Bogdanov who pointed out that that what was involved were goods produced by branches of the particular activity and that their primary feature was the temporary, ephemeral and fleeting aspect of their outlets.

It is evident that once Kondratieff chose particular goods in order to verify the existence of wave-shaped movements in the essential capital goods, he inevitably opened himself up to the criticism that he was discovering life cycles (of a product or of a sector) where what he was claiming to find were activity cycles. By extension, Oparin thought that what was true for the railways was equally true for the series dealing with coal mining or iron production, because "*all the progress of technology consists precisely in using the least possible energy and materials, and obtaining a maximum effect*." The effect on the curve in this case would thus not be a downward trend in production in absolute terms, but a clear slowdown of growth, which might erroneously be interpreted as indicating a period of stagnation. Making an argument which combined the absolute effects (saturation of markets) and relative effects (relative saving in materials) of eco-nomic development on the essential capital goods and on the conditions of energy consumption equated to denying their capacity to generate long movements of the "large cycle" type, and thus to denying any basis in material terms for such hypothetical movements.

Thus, the angle of attack chosen by the critics to challenge the notion of essential capital goods does not permit of any questioning of the functions of the latter in the mode of development of economic activity because it observes the additive method of Kondratieff. The complex links which obtain between the technical and social division of labour on the one hand and, the extension of the means of circulation and transportation as well as the increased mastery of production and distribution of energy on the other are not considered or discussed with a view to proposing a series which would be more significant than one comprising only the railways, roads or coal. Kondratieff, in his reply to Bogdanov, while reaffirming his initial positions, does however introduce two interesting clarifications which lean in that direction. The first involved emphasising that the 50-year rhythm was created by a coming together of a "*radical renovation*" and an "extension" of the capital goods, thus explaining the periodic return of the need to assemble a significant mass of capital, which also had the effect of refuting the argument of the saturation of markets over time. The second pointed out that the list was not final, "*the essential productive forces (not being able) to be always and solely restricted to the rail-ways and the installation of factories*." However, these clarifications did not cause him to draw up other variables, nor to take his analysis to any greater depth.

The introduction of the technological innovations (implied in the expression "*radical renovation*") as a complement to the criteria of wearing-out time and mass of financing offered Kondratieff's opponents a new angle of attack for their criticism, relating to the logical relationship between cyclical movements and technological revolutions. They all thought that the relationship established was false, but with varying arguments. One group (Bogdanov and others) thought that "*the technical revolutions are chance interactions between the development of scientific thought and economic dynamics*." By contrast, while the second group, within which Oparin developed the most complete arguments, did not question the hypothesis of the type of relationship established by Kondratieff, it did contest the idea that it can take place in the context of the "large cycles" which he claimed to have found. The first argument involved affirming that the ten-year cycles constituted the natural framework of diffusion of technological innovations, with upgrades of equipment in the rising phases incorporating the innovations accumulated in the depressed phases. This was a general rule verified for each one of the cycles and well established by theory. Consequently Kondratieff's hypothesis on the accumulation of the inventions during the descending wave of the large cycle could not be correct, because it assumed a *contrario* that the medium-sized cycles of the large cycle's rising wave would not see any innovations, a situation which had never happened. As a complement to this, the second argument involved saying that it was not conceivable that inventions could remain unapplied for several decades if they offered the possibility of lowering production costs.

The critics saw here a major contradiction in reasoning, one which Kondratieff refuted vigorously. In his responses, he refined his analysis by indicating that the greater or lesser degree to which the innovations were radical differentiated them from one another, although unfortunately without providing much more information on the descriptor used. He deduced from this that the movement of diffusion of the radical innovations could not be linear because it was constrained by the conditions of profitability of the major amount of capital needed, not by a simple maintenance or refurbishing of the essential capital goods ("*renewal part by part*"), but by their complete recasting. The impact of this constraint of profitability varied with the types of replacement entailed in the investment expended. Consequently, the process of diffusion of the innovations was typically heterogeneous over time, with at the same time an improvement in processes of an existing technology and introduction of processes from a radically new one. But only the radical new technological innovations were concentrated in the period of time corresponding to the upturn of the long cycle, the others being dependent on the conditions specific to the ten-year cycles. The logic of the demonstration was interesting with respect to its approach to innovation and provided an effective counter to the preceding criticisms. But from the point of view of the method of analysing the large cycles which concerns us here, this response leads one to think that the radical re-equipping cannot be limited to the essential capital goods only, but that it also impacts all of the means of production, whatever their degree of wear or financing. This approach to the question assumes that the essential capital goods would be the precondition for the generalisation of his famous radical new technological innovations over all of the means of production. Here too we return to the earlier question on the understanding as to the process of extension of the social and technical division of labour.

Following our examination of the nature of the essential capital goods and then of their renewal time, we have to express our doubts about the definition given by Kondratieff to the notion of wear, because of the vagueness surrounding it. The first definition ("*material productive forces with a long working life*") refers to the length of time that the equipment is operational, its amortisation period (50 years) then corresponding to its physical lifetime. Nothing in Kondratieff's presentation gives any indication that would allow a different approach to the problem of the wearing-out of the essential capital goods. The expression ("*the basis in material terms of the large cycles is the wear*") therefore has to be taken in its primary sense: the basis in material terms is physical wear. Bogdanov accepted this interpretation but disagreed with how far-reaching Kondratieff made it, above all because he combined it with the idea of a concentration in time of the investments. That would actually equate to saying that once the essential capital goods had been produced and installed, they remained in the same state for the entire time that they were operated without any "*new enlargement*" to respond to the increases in production generated by "the medium-sized cycles in their rising curve" and without "*repair or renewal part by part*." The different fragmentary responses which Kondratieff gave[66] could give rise to the impression that this criticism caused him significantly to modify the concept of wear, by substituting the concept of technological wear for that of physical wear, which might possibly lead to that of obsolescence. What is the real situation?

66 "This objection would be correct if I had stated that there exist material productive forces which are partially renewed only at the end of 50 years, and that in the interim they must remain absolutely unchanged;" see also the argument on "the radical renewal" considered above, Kondratieff 1992d, p. 285.

In fact, the clarifications contributed by Kondratieff tend rather to be sources of additional confusion. He continued to maintain two different explanations of technological wear for the rhythm of renewal of the essential capital goods. In one of them, this is determined by the time for which they have been physically wearing out, while in the second it depends on the rate at which the capital necessary to finance them is collected. One or the other: either the similar duration of the two "large cycles" revealed (approximately 50 years) is due to chance, and in that case the endogenous explanation sought will not be found, or else the two are linked, and if so, allowance must be made for it. The responses given to Bogdanov do not reveal to us how Kondratieff resolved that question, which was all the more important in that he had to explain why, at the end of the process, there is <u>also</u> a radical change in the nature of the essential capital goods.

3.1.2 The second approach tends to correct the gaps and inadequacies of this first definition, by stressing, correctly, the nature of the technology being implemented and the forms of organisation of economic activity on which they depended. He then introduced a new dimension into the analysis: the succession of the large cycles represented a "*radical technical re-equipping*" at more or less regular intervals and corresponded to "*organic changes in the economic system itself*" and to "*a new regrouping of the principal productive forces of society*," each cycle starting at a level of productive forces higher than the one before. This juxtaposing in the same explanation of a "*radical technical re-equipping*" with the "organic changes in the economic system itself" was the beginning of the contemporary theories of regulation, which one does not find in such an elaborate form in Kondratieff's contemporaries[67].

This approach *explicitly raises the principle of qualitative and quantitative leaps* of which the "wave-like movement" of economic activity are no more than a manifestation. Which also equates to saying that all of the period corresponding to the total duration of one cycle (about fifty years) has to be characterised as a trend towards establishment of a certain homogeneity and a certain stability in the technology used, and thus in consequence manifesting itself by a particular mode of use of the labour force, by the relative importance of the sectors (in terms of price ratios or weight in the total production, etc.) and by the nature and scale of the essential capital goods. Kondratieff then concluded the list of the latter by including structural elements such as, for example "*the training of management and of qualified manpower*," the "*reorganisation of the relations of production*" or "*a better organisation of companies*."

He did not seek to produce series which would make it possible to determine the movement, such as, for example, series dealing with manpower employed (numbers, proportion of the wage-earners, qualifications, etc.), as Potyagin[68] very correctly noted during the arguments. Kondratieff accepted the criticism, remarking that his "*work was still far from being concluded*." He would limit himself, therefore, to basing this supplement to the definition of the essential capital goods on the empirical regularities which he had deduced simply from observation of the movements of the past.

This second aspect of the definition of the essential capital goods also drew heavy critical fire, for several clusters of reasons.

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67 The following long quotation gives an indication of this originality: "sooner or later a moment occurs in which a major investment in large items of equipment entailing radical changes in the conditions of production becomes sufficiently profitable. For any given historical period, then begins a phase of new and relatively spectacular construction in which the accumulated technical inventions are widely applied and in which new productive forces are created. (...) At the same time, the tumultuous growth of new productive forces, stirring up the activity of the classes and groups taking part in it within the country, creates favourable conditions for the fight against outdated socio-economic relationships which retard development, conditions favorable to large internal upheavals. This is why, as we have seen, a period of prolonged rise of the business cycle is linked, in reality, to radical changes in the production sector."

68 Ibid. p. 238.

3.1.2.1 The first controversy concerns the method of determining the dates when the large cycles changed direction. He naturally rejected the method consisting of determining them in relation to the upturn or downturn the corresponding short cycles because there was no theoretical reason for that to be the case (although that is a method which we find in Tugan-Baranovsky and in Spiethoff). Nor was he able to base himself on the curve of the smoothed deviations from the theoretical series to the empirical series since the latter, having no other function than to reveal the existence of "wave-shaped movements," do not express the real movement.

Nor do the explanatory details added by Kondratieff to the definition of the essential capital goods make it any more possible for him to establish accurately the dates when the large cycles change direction, because fundamentally the nature of the process which he suggests forbids him to do so[69]. This is an extremely important position which is virtually never mentioned by the authorities referring to Kondratieff's work. He therefore proposed an interval of a few years around a date read directly from the empirical data. Oparin admonishes him for this way of proceeding: the consequence of making the dates so approximate, which in his view made them almost arbitrary, would be that shifting, reducing or increasing the periods of reversal by a few years would be enough for the empirical regularities revealed by Kondratieff to be modified and rendered meaningless[70]. It is true that Kondratieff's method for determining the dates of reversal is weak and results in a certain circularity in the reasoning: the dates are defined only from empirical laws which, themselves, cannot be established unless those dates are determined.

3.1.2.2 The second dispute related to the meaning of the upturns and downturns of the long-period business cycles. It was not so much the idea of slicing up economic history into periods which was surprising. At first glance, this appeared perfectly compatible with Lenin's scheme on a succession of "stages." In fact, his idea had nothing to do with that, and that was the cause for complaint. Instead of describing a continuous and linear movement of capitalism, with its rhythm given only by the periodic industrial crises, each of the successive stages of which corresponded to a further degree in the concentration and centralisation of capital, a movement at the end of which the system would necessarily be bound to collapse, Kondratieff presented a capitalist system capable, "all other things remaining equal," of overcoming its periods of depression and engaging in a new long period of expansion under renewed social and technological conditions. Consequently, in the eyes of certain Russian Marxist economists, Kondratieff's argument amounted in effect to a negation of "the law of the growth of the productive forces" (as Bogdanov said) and even more seriously to an apologia for an everlasting capitalism[71].

This polemic did not make him try to increase the degree of detail in his analysis of the pair consisting of the level of the productive forces and the forms of the accumulation which he considered, however, to be at the heart of the issues related to the large cycles[72]. Instead he recalled his methodological premisses because

69 See his reply to his opponents: "These are extremely complex events, and an extremely com-plex problem, and they would all wish that one could show them a virtually mathematical co-incidence of the phenomena, a coincidence as transparent as crystal, of all the fluctuations of reality with the general pattern" and to Oparin in particular: "I noted in my presentation that it was extremely difficult, at the present stage of the research, to determine exactly the beginning and the end of the periods of rise and fall in the large waves," (ibid., p 274)

70 Oparin was thinking more particularly of the first law (on technical progress) and the second (on socio-political upheavals).

71 This criticism of making an apologia for capitalism was aimed at him very early following the publication of his work on the post-war economic situation (cf. *Discussion of Questions on the World Economy and of Crisis*, Kondratieff N.D. 1992f), i.e. well before his work of 1925 and 1926. The ciriticism was never truly supported by arguments dealing with the fundamentals of Kondratieff's conception, as is shown by this dogmatic expression of position by Spectator at the time of the discussion at the Institute for Business Cycles: "I think that this reference to Marx and Engels is sufficient on its own to excuse us from having to discuss this question (of the large cycles) with Professor Kondratieff, as it is highly unlikely that there will be any among us who would be prepared to defend the point of view of N.D. Kondratieff against Marx and Engels!" (ibid., p. 244).

72 See his response to Bogdanov: "But if he knows Marx, he cannot be unaware that, in his view, the growth of the productive forces under capitalism, which expresses itself by their increased reproduction, assumes accumulation. In other words, accumulation is one of the key moments of the process of development of the productive forces, whereas comrade Bogdanov is quite simply contrasting, without any critical approach, the one and the other process, in other words to some extent contrasting a part with the whole. Obviously, it is not sufficient to speak of the growth of the productive forces, it is also necessary to analyse this process," Kondratieff 1992d, p 285.

he was convinced, as he said in his response to Ossinsky in 1924, that his position and those of his critics "are located to a large degree on different planes of thought[73]." The theory of the large cycles within business is situated on an intermediate level between the analysis of the concrete historical development of the capitalist economy (where he places the positions expressed by Trotsky, Bogdanov, Ossinsky, and so on) and the formal theoretical analysis of the crises (like those of Marx with his schemes of reproduction). This intermediate level is called by Kondratieff the dynamic analysis of the business cycle. Its function is to determine the general laws governing development of economic activity which are repeated over time "all other things remaining equal," or, to express it as he would, *caeteris paribus*. These laws describe the reversible mechanism of the large cycles understood as being subject to repetition irrespective of the historical context of the concrete process of development which, for its part, is irreversible. He states therefore that "it must not be forgotten that each cycle takes place in specific new historical conditions, at a new level of development of the productive forces, and for that reason is not at all simply a repetition of the preceding cycle." In consequence, he thought that a major part of the criticisms reflected a lack of understanding of that necessary methodological breakdown of the analysis of the process of development into those three levels, a lack of understanding which was responsible for the numerous misinterpretations of his theoretical project and his proposed explanations.

This approach of Kondratieff's, however, did raise a number of questions two of which are particularly interesting: 1- How does the idea of a "harmonisation" between forms of social organisation and the technical constraints of production[74] fit together with the concepts developed elsewhere of contradiction in the relationships between private capital and sectoral disproportionalities? 2- Why and how are the shocks received from the outside (wars, social unrest, discoveries, etc.) made endogenous? It is then astonishing to note that this aspect, formalised more or less explicitly in Kondratieff's presentations, evolves, at best, into a discussion on the fortuitous, random or exogenous nature of concrete events and on the logical antecedence of the social upheavals relative to the major turning-points of the business cycle. While it may be noted that Trotsky tried to place himself alongside Kondratieff when he stated that it was possible to detect "*interrelations between the clearly drawn epochs of social life and the clearly marked segments of the curve of capitalist development*," by reducing "social life" to the "super-structures" (understood as ideology, currents of intellectual thought, the nature of the political parties, etc.) he completely voided the theoretical aspect of the analysis of the long business cycle[75] described earlier. Kondratieff can then allow himself to admonish Bogdanov, Trotsky and the other opponents that they were taking up "*an idealistic point of view*" rather than a materialistic one[76].

3.2 Free capital and the rhythm of development

The basis in material terms of the large cycles was thus considered to reside in the conditions of renewal of the essential capital goods. Initially, Kondratieff defined these from the physical point of view. Subsequently, he sought to explain why their accumulation took the form of a discontinuous movement by studying the conditions under which they were financed. At the same time he sought to explain their effects on the remainder of economic activity. At the conclusion of the study, he ought to have been able to reconstitute the reversible mechanism ("reversible" in the sense of reproducible) of the large cycles. He based his demonstration on the interrelationship between essential capital goods and free capital. This latter concept was introduced

73 Kondratieff, 1992f, p 524.

74 In 'The Dynamics of Agricultural and Industrial Prices,' he even talks of the establishment of "norms of consumption," Kondratieff 1992e, p 417.

75 Trotsky L.D. [1923], "The Curve of Capitalist Development" (translated into French), in: *Critique de l'Economie Politique, Maspéro*, Paris, no. 20, April-July, 1975, p. 8.

76 Kondratieff 1992d, p 286.

into the analysis at a late stage, but in fact it constituted the heart of it: it tended to explain both the existence of a major and long-lasting accumulation fund to finance the essential capital goods and at the same time the profile of the long cycle which resulted from it. From the methodological point of view, that means that the presentation of the explanatory scheme could not be understood as simply a comparison between statistical results and the observation of empirical regularities, since Kondratieff, at the end of his presentation, was to introduce a new graph (balances of deposits in savings banks) and undertake a succinct analysis of the evolution of the forms of concentration and mobilisation of capital. Oparin rightly pointed out that "*to give a basis to his theory of large cycles*," Kondratieff had implicitly to introduce "*a fifth law*," a remark, moreover, which Kon-dratieff did not contest.

3.2.1 Renewal of the essential capital goods is costly and takes a long time.

It cannot take place unless the context is favourable to an ongoing mobilisation of major quantities of capital. The triggering of this process of renewal operates only when pre-existing capital chooses productive investment as the principal way that it should be utilised. This capital constitutes what Tugan-Baranovsky calls "*available savings*" and Kondratieff calls "*free capital*."

The hypothesis marshalled into the reasoning here is thus a strong one: the conditions of economic development are such that not all the incomes generated and distributed by it are necessarily re-invested productively or transformed into individual consumption. We are back at the traditional criticism of the law of outlets developed by Tugan-Baranovsky.

Right from the start, on this particular point some took exception to the explanation, given that "*free capital can never exist in society*" (Kreynin). The reply to that argument involved demonstrating that the assumption that all branches of the economy were in a state of adequacy, which is a consequence of this absence of free capital, postulated that the economy was in a situation of equilibrium on a perpetual basis, but then it would not be possible, for example, to understand the long and regular movements of prices and interest rates. We are faced once again with the traditional conception of Kondratieff on the dynamic business cycle.

Almost all of Kondratieff's opponents noted this closeness to the theories of Tugan-Baranovsky, and would reproach[77] him for it all the more violently in the context of the year 1926 since the latter was a minister in the last government of Kerensky. Concerned to defend the memory of the man whose pupil he had been, he claimed this family tie without ever expanding, in this debate, on the fundamental differences which he had with him[78]. Indeed, it may be noted that Kondratieff in his explanation of the large cycles was not interested in the problem of the equilibrium between quantities produced and quantities consumed under the habitual twin constraint of the structure of the supply and the structure of the disposable income, which is precisely what Tugan-Baranovsky was seeking to do in the context of a "genuine" theory of outlets.

Kondratieff's argument led to the conclusion that the origin of the large cycles was to be found in the relation between the total mass of the free capital and the portion of it invested productively.

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[77] For example, Spectator explains that "the point of view of Kondratieff on the crises is that of Tugan-Baranovsky. But he has not even been able to link the large cycles to that theory." (ibid. p 244). Bogdanov develops the same point of view: "when the author developed (this) argument, I thought, and many of our comrades also, of Tugan-Baranovsky" (ibid. p 247) as did Falkner: "his [Kondratieff's] thinking, his theory, exactly formulated is no more than a transposition of the theory of the usual capitalist cycles of Tugan-Baranovsky to the phenomena of the large waves in the world-wide movement of prices," (ibid. p 254).

[78] "It is true that there exists a certain link between the conception of Tugan-Baranovsky and mine. But it is also true that it is not a question of a simple transposition of Tugan-Baranovsky's theory. I consider his idea on the accumulation of "free capital," and on the role of this accumulation, to be very fruitful. For the rest, my conception is profoundly different." (ibid. p 291). He wrote an article on Tugan-Baranovsky shortly after his death, which unfortunately has not been published in the West (Kondratieff N.D., "Michail Ivanovich Tugan-Baranovsky," in *Izdatel'stvo "Kolos*," Petrograd, 1923).

The expansion phases corresponded to the periods during which the portion of the free capital which it was intended to invest productively remained for a long time higher than the demand for investment, or in other words "*the curve of the rhythm of accumulation is higher than that of investment*." It should be noted that the reasoning is always expressed in relative terms, and that Kondratieff did not give any information on absolute volumes. He noted this problem in his presentation of 1926[79] and deduced from it that it was necessary to construct a series "*concerning the issue of new amounts of capital (but) unfortunately, these data do not exist for a sufficiently long period*." He therefore proposed a series showing the evolution of the deposits in the Caisse d'épargne savings banks in France. This series was supposed to illustrate the evolution of the amounts of monetary capital not re-invested productively, even if this type of deposit represented no more than a fraction of those amounts. He assumed that its movement was the reverse of the movement of the other portion which was invested productively[80]. His analysis demonstrated that there was indeed a reverse movement relative to those of prices and of interest on capital thought to be in phase with the amount of the productive investment. He deduces therefrom that "*it is really at the moment at which the descending wave of the large cycles reaches its lowest point, that the accumulation of the free capital reaches its maximum pressure, and vice versa*." He thus interprets the high level of the balance of the deposits as being correlated with the low level of interest rates and thus in consequence with the low level of productive investments.

Obviously, Oparin contested these conclusions. He stressed that the series proposed was insufficient, that at least equivalent English and American series should be blended into and that in any event the proposed analysis of his graph (no. 12) was fallacious because no cycle could be detected.

79 Kondratieff tackled this question for the first time in his presentation of 1926, but it does not appear in the German version of that presentation.

80 To those who complained of this approach, he countered with the following argument: "I have studied the essential features of the data available to us, for example on the issue of stocks, changes in joint stock companies' capital in certain branches of industry, etc. These data relate to an insufficiently long period, and that is why I have not considered it necessary to quote them" (ibid. p 279).

Figure 1 Private Savings Banks in France
Balances of deposits at 31 December each year
(millions of francs)

see figure:		
Serie brute	=	Raw series
Serie théorique	=	Theoretical series
Ecarts …..	=	Deviations from the theoretical series
Ecarts lissés	=	Smoothed deviations

More fundamentally, he put forward a much more traditional monetarist argument, namely that the increase in the means of financing available to productive investments does not mean the prior existence of amounts of free capital, but rather a more extensive system of credit and a more pronounced monetary circulation or "*in other words*, the extension of the means (of payment) corresponds to the increase of the needs of business."

Complementing this argument, Falkner had doubts about the origin of the remuneration of the free capital not invested productively, given that it is precisely the productive capital which make it possible to finance the interest paid out. It was therefore inconceivable that such sums of capital might remain idle for some decades rather than being rapidly invested in industry. He deduced from this that Kondratieff's hypothesis was "*absurd*." However, immediately after this and in a total contradiction with this argument, he conceded the "*incontrovertible*" existence of a contracyclical movement in the deposits at savings banks, the explanation for which could be found in the fact "*that in a period of depression the ratio of the amounts of capital invested in industry is markedly lower and more problematical, and it is for this reason that a certain portion of resources is invested in the savings banks to bring in an income.*"

3.2.2 The nature of the criticisms formulated by Oparin and Falkner directly confronted Kondratieff with the question of the forms in which the free capital existed and was reproduced.

His first definitions remain vague and ambiguous ("this accumulation may take place partly in kind, partly in money in the widest sense of the term"). Initially, we may indeed think that he is reducing the free capital to an accumulation fund with finite limits conserved in the form of investments in traditional securities (liquid savings, Treasury bonds, pension fund, etc.) or speculative holdings (gold, etc.) while waiting for the right moment to make more remunerative investments. This approach was all the more plausible for Falkner and Oparin in that it was Tugan-Baranovsky's. From that point, the criticisms veer off towards a search for the social strata whose type of fixed incomes would put them in a position to release the savings which would then feed into the stock of free capital (persons on fixed incomes, property-owners, and so on). This view seemed all the more convincing to them in that Kondratieff refers explicitly to such strata. They were then concerned to demonstrate that the final cumulative volume would be insufficient to finance the major investments which Kondratieff described as the causes of the rising wave. This volume in their view would be all the more insufficient, Oparin explained, in that the persons on fixed incomes could not be the originators of this fund, because since the interest rate was dropping at the same time as prices, their purchasing power would not improve.

In fact, Kondratieff's idea is more complex.

He readily recognised that the amount of free capital available at the end of the descending wave was insufficient to take care for more than a decade of the renewal and extension of the essential capital goods in their entirety[81], but he continued to consider it logically necessary that the capital should grow before that process of investment started. What justified the upturn of the long business cycle was thus not, in itself, the prior existence of a significant level in the fund of free capital, but the engagement of <u>a process of rapid and continuous reconstitution</u> of the fund. Kondratieff was explicit on this point: it was necessary "*that the process of accumulation should persist, and persist at a rate such that its curve is higher than the curve of the current investments*." The approach was thus much more consistent with his analysis of the dynamic business cycle.

The whole question now came down to ascertaining what was the motor of this process. On this point, at the time of his presentation in 1926, he proved to be particularly evasive in putting his explanations off "until later." His replies to Falkner, whom he accused of having an interpretation that was too literal and too mechanical, did allow a number of important points to be clarified:

- Firstly, he understood by free capital, "*an amount of capital offered in such large quantities that it becomes less expensive*," in other words that the rate of interest becomes lower than the profitability of productive investment. That situation cannot last unless the increase in the investments generated by the low rate of interest does not result in a reduction of the mass of capital on offer, because in that case, the interest rate, becoming once again higher than the profitability of the productive investment, would halt the movement. It was therefore necessary that the causes of the reproduction of the free capital should be structural and not only fleeting.

- The second clarification concerned the form that this free capital takes. He said that trying to compare the cumulative amount of savings released at the time that investments are made, and to deduce therefrom that the deviation observed invalidates *de facto* his proposition that there is a link between essential capital goods and free capital, in fact reveals a gross lack of understanding of the nature of

81 "However large the accumulation which has already taken place, there will never be a fund of capital so large that one may go on spending it for a decade or more," Kondratieff 1992d, p. 159.

the banking system. One of the particular features of the banking system and of the stock exchange is that they are able, by centralisation of savings, to make available an amount of credit that in fact exceeds demand, thus pushing down interest rates.

This centralisation was all the more necessary "*in that the sources which provide the loanable funds are very diverse.*" They were not limited to the list drawn up by Oparin. The industrial undertakings and the banks belonged on the list also. This means that Kondratieff was shifting the question to the more general level of the distribution and formation of all the incomes and not only those of the people on fixed incomes, thus making it clear how he differed from Tugan-Baranovsky. In this connection, and contrary to the opinion of Oparin, he maintained that "this group can also be the source of an increased supply of loanable funds" because the interest rate curve and the price curve are not perfectly synchronous. According to his observations, they are slightly offset in time and in amplitude. There was the same type of reasoning for companies. Thus, at the beginning of the upward period of the long business cycle, the participation of the industrial companies in constituting the free capital resulted from an improve-ment in their relative prices owing to the depression rampant in agriculture[82].

It then becomes difficult to go with Kondratieff when he places all of the sources for constituting free capital on the same level. In fact, the situation of those on fixed incomes does depend on variables (prices and interest rates) over which they have no control, which is not the case for industrial companies. In consequence, if one accepts the reasoning of Kondratieff, only the relationship between industry and agriculture is essential to the explanation for the mechanism of the large cycles[83], because by determining in the final analysis the capacities for productive investment, it has an influence on the evolution of their respective nominal price levels, and thus triggers the process of reversal of the movement of the relative prices, which is synonymous with the upturn or downturn of the long business cycle depending on the sector profiting from the reversal. But at the same time, this analysis no longer makes it necessary to draw up a theory of distribution.

His approach to the industry / agriculture pair is based on three very important postulates:

- The first is that the principal effect of the investments made in radically new technological processes, in particular in the essential capital goods, is to cause a lowering of the costs of production. This is the essential factor for an improvement in the profitability of companies, which is a primary condition for the engagement of a continuous process of upturn in the long business cycle since it is the ratio between the rate of interest and the rate of industrial profit which determines the decision to undertake the costly investments in essential capital goods. Evidently, this principle of increase in productivity holds true whatever the sector of activity, but it is less pronounced in agriculture, since the latter is subject to the law of diminishing returns[84].

- The second postulate explains that one of the effects of the essential capital goods is to prolong the period of expansion at a time when the process of renewal and extension of it has already reversed. Kondratieff did not supply any part of an explanation on this phenomenon, contenting himself with tal-

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[82] "The industrial and commercial groups themselves are also centres of accumulation, to the extent that the agricultural business cycle remains less favourable, during the descending wave," Kondratieff 1992d, p 282.

[83] "Under these conditions and in the absence of external circumstances, the large cycles within business are accompanied precisely by this organic regularity in the fluctuation over time of the purchasing power of agricultural and industrial products, that we have discovered," Kondratieff 1992e, p 461.

[84] If one accepts Kondratieff's reasoning in 'The Dynamics of Agricultural and Industrial Prices,' "agriculture is subject to the law of diminishing returns. (...) The influence of technical progress on lowering the costs of production is inevitably weakened by the consequences of that law," Kondratieff 1992e, p. 417.

king of inertia[85], whereas what was needed was to define the conditions in which the reproduction of share capital takes place, by stating in particular the link which exists between physical wearing-out and economic obsolescence of the essential capital goods. In fact, the remark acts to confirm the idea that it is not possible to date with precision the turning-point of the long business cycle: the precise details of the forms and conditions of that turn (in this case, downwards, but he thinks the same of the upturn) are constrained by the actual socio-historical context and not by an automatic link with economic variables.

- The third postulate puts forward the principle that industry has a greater capacity for adaptation to the turning-points of the business cycle than agri-culture, both in terms of adjustments of the quantities offered to the de-mand ("*agricultural production is less flexible than industrial production and is not able to increase or reduce its volume as rapidly*") and in terms of adaptation of structures. The shocks impacting agricultural prices, tending upwards in a period of expansion or downwards in a period of recession, re-sult in major deviations in evolution, with respect to industrial prices, thus generating transfers of income from the one sector to the other. But these transfers are not on a comparable scale; they are to the benefit of industry at the end of a descending wave and in particular during the ascending wave, when activity reaches its highest point, whereas agriculture manages to reverse the movement a little before the downturn and during the period of depression, when activity is more reduced[86].

The notion of free capital takes on, under these conditions, a very different interpretation from that of Tugan-Baranovsky and from that which is found in the traditional presentations of Kondratieff's theory of large cycles. Indeed, it is very clear that in no case can it simply be reduced to the availability of savings. Kondratieff is clearly describing a process of reconstitution of an accumulation fund linked to a general upward movement of the productivity of labour due to a recasting of the productive capacities and to a recomposition of the forms in which society and economic activity are organised.

One of the ways in which this fund appears is of course the savings of households, but the dominant form is constituted by the increase in the financial margins of the commercial and industrial companies and the banks. Taking into account his analysis of the relationships between industry and agriculture, this amounts to saying that the origin of the reconstitution or of the drying up of the accumulation fund is external to the internal mechanisms of the accumulation of capital. As an example, it is evident for Kondratieff that the general upwards movement of prices, synchronised with that of the business cycle, does not express difficulties in obtaining value for the capital, difficulties which one might link, for example, to an excess of capital or to a settling-down of the gains in productivity, but is the induced effect of the increase in agricultural prices. The effects may be direct (increase in the cost of supplies) or indirect (reduction in the quantity of capital available, resulting in its becoming more expensive). The analysis can thus be developed without encountering any need for recourse to a theory of distribution, since all that is needed is to know at what moment industry finds itself in a position to profit from transfers of value arising out of a situation unfavourable, in cyclical terms, to agriculture.

3.2.3 Kondratieff was now in a position to produce a general law on the rhythm of secular development of the capitalist economy, a law which is richer than the way it is traditionally presented. He sought to determine all of the elements which logically go to make up a cyclical movement of long duration, all other things remaining equal. The law could be stated as follows.

85 "As the ascending wave appears following major accumulation activity, and long-term invest-ments in expensive basic equipment, a relatively long period goes by before the inertia of the movement is absorbed and the descending wave starts," Kondratieff 1992d, p. 161.

86 See the analysis developed in Kondratieff 1992e, pp. 458 and ff.

3.2.3.1 The favourable evolution, starting from the middle of the phase of depression, of relative industrial prices, permits the release of a growing flow of free capital but the low level of the rate of profit, explained by a slow deterioration in the conditions of production, makes productive investment not very attractive. These dispersed amounts of capital essentially are in monetary form but their regular growth will bear down on the rate of interest which is moving downwards, and will at the same time cause initiatives of concentration. As soon as the rate of interest becomes lower than the rate of profit, the conditions obtain for a reorientation of the free capital towards productive investment or in other words "*the development of the descending wave gradually creates the preconditions for a new durable rise.*" Certainly, these are preconditions and not sufficient conditions, because as Kondratieff says, "*this increase, of course, is not inevitable,*" since "*the organic changes in the economic system itself may distort the character of the economic dynamics. But if that does not happen, the descending wave will be followed by a recovery.*"

This point is essential: <u>the upturn is not automatic</u>, since it is dependent on ancillary conditions which are partially related to expectations concerning the maintenance of a favourable business cycle (level of profit and significant mass of not-too-expensive capital). By hypothesis, taking into account the structural aspect of the transfer of value between agriculture and industry, Kondratieff thought that those conditions should obtain. Then "*sooner or later*" taking into account the medium-term prospects for maintenance of a rate of profit higher than the rate of interest, there is created in society a climate favourable to a major and continuing renewal of the essential capital goods "*resulting in radical and profitable changes in the conditions of production*" by application of the technical inventions accumulated and not implemented during the preceding period of depression.

The necessary and ancillary condition thus resides in the expectation brought about by the "powerful decision-making centres" on maintenance of favourable conditions. This way of raising the problem of the upturn may make one think of the Keynesian under-employment equilibrium, to which, moreover he refers in *The Dynamics of Agricultural and Industrial Prices* when he considers, although without developing the idea, whether it might be possible for the economy to develop without fluctuations in the business cycle[87].

3.2.3.2 If these different conditions are brought together, then an ascending wave of the business cycle begins.

The massive utilisation of the free capital for the extension and the radical renewal of the essential capital goods stimulates economic activity as a whole. The reproduction of the free capital is then provided by the movement of accumulation itself, because of the drop in the costs of production which it generates. But, "*in the internal conditions of its development will also be found the reasons which prevent it from being prolonged indefinitely*." Agricultural prices increase, keeping pace with the extension of industrial production and with the growth in the working population and its income, but to a higher degree than industrial prices. The increase in purchasing power of the agricultural products which results, by reversing the flow of the transfer of value, leads to "*an influx of relatively greater resources into the hands of the rural population, which to some extent limits the possibility of the upward wave in industry continuing for a long time.*"

The moment of the reversal is not predefined, but Kondratieff tells us that it occurs, in fact, following the beginnings of a rise in the price of the free capital following on from the major drain from it brought about by the massive investments in essential capital goods. These investments stop, retarding expansion to a compara-

[87] The reference is to chapters I and IV of his 'A Tract on Monetary Reform' of 1923 (Kondratieff 1992e, p 383] in which we read: "under current conditions, the intensity of production is largely determined by the real profit for which the entrepreneur is hoping" (incorporated in: Keynes, *Essais sur la monnaie et l'économie*, Paris, Payot, 1971, p. 32). Kondratieff corresponded with him, but to date the correspondence has not been published.

ble extent. The ascending wave had initially combined with an extension of the world market by the inclusion of new countries. But the accumulation of the factors tending towards a reversal of the business cycle would subsequently manifest themselves in an exacerbation of competition and various conflicts of interest, increasing social, political and economic instability. "*Inevitably,*" the upward movement would reverse, as soon as there were confirmed and steady prospects of a level of profit lower than the rate of interest. The reduction in industrial activity would then bring about a drop in demand for agricultural products, but this drop would be much faster than the reduction in supply. The result would be a downward trend in agricultural prices which would be faster and more severe than the drop in industrial prices, and thus in the fullness of time a new reversal of relative prices to the advantage of industry would occur. The process of reconstitution of free capital for productive investments would then be able to restart.

From a formal point of view, this explanatory scheme of Kondratieff's is based fundamentally on the existence of expectations concerning the evolution of the rate of profit by the "decision-making centres" and of differences between sectors from the point of view of their reaction time to the changes in the business cycle and their capacity to improve their conditions of production. Only the combination of these elements makes it possible for Kondratieff to produce a reversible law of large cycles without recourse to a theory of distribution associated with his theory of accumulation. At the beginning of his article 'The Dynamics of Agricultural and Industrial Prices,' he recognised this clearly: "*indeed, if (leaving aside purely accidental deviations) the changes of state of the various sectors occurred in perfect harmony*" the large cycles would not exist. It may be said in consequence that if the essential capital goods are the basis in material terms of the large cycles, they are not the explanation for them.

3.3 An internal contracdiction in Kondratieff's explanation

Kondratieff claimed to be providing an endogenous explanation of the process and the rhythm of development of the capitalist economy. Regardless of any theoretical bias[88], can one consider that he succeeded? We do not think so. Indeed, despite the rigour with which he makes his demonstration, he has to introduce explanatory elements from outside the economic sphere and has to assume that relations between agriculture and industry are stable.

The heart of his analysis comprises the explanation of the pendulum-like movement of the transfers of value between agriculture and industry, sometimes to the advantage of the first (at that point, prolonged periods of depression start), sometimes to the advantage of the second (entailing at that moment long periods of expansion). These transfers of value occur by means of the evolution in opposite directions of the purchasing power of their respective products. While they express a deterioration in the purchasing power of the products of a given sector, or in other words a deterioration in their relative prices, they are not explained in Kondratieff by the evolution of the respective conditions of utilisation of the capital which had made their creation possible. He refers neither to the internal processes of accumulation of share capital, nor – and this is obvious – to the conditions under which the capital is utilised in industry. We thus find ourselves faced with a real problem of closing the loop of the system of determination of the internal mechanism of the "large cycles," because the determination that there are alternate reversals of the direction of evolution of the relative prices of agriculture and industry is not enough to act as an explanation.

In order to remain consistent with his project of elucidating the question of the large cycles, Kondratieff would have to produce an explanation combining the internal mechanisms of operation of the two sectors with those

[88] As for example that of invoking the simple absence of any reference "to the fluctuation of the rates of profit" to void the analysis produced by Kondratieff, as E. Mandel does in The *Third Age of Capitalism*, 3rd edition of *Der Spätkapitalismus*, Union générale d'éditions, 10/18, Paris (3 volumes), 1976, p. 277.

of the linkage between them. However, he invokes only the natural and social conditions weighing on the mode of operation of agriculture. As we have seen, that is what ultimately determines, by limiting the capacities for adaptation and reaction of agricultural production, the foundation of the pendulum-like movement of the evolutions in purchasing power of the agricultural and industrial products, and thus of the large cycles, as Kondratieff says explicitly: "*it is indispensable to give consideration to certain particular features specific to agriculture, by contrast with industry.*"

These particular conditions controlling production in agriculture are of two orders: *relationships of ownership* entailing the use of the land and thus the diversion, via the rent, of a portion of the agricultural income (and thus of the income of society) and the *conditions of the process of agricultural labour* itself, arising out of the fact that this production is "organic" in character[89]. Even if what is involved are not fleeting or random events as postulated by his opponents (such as war, technical discoveries, new veins of gold, and so on), they are still exogenous elements, even though they may be structural and permanent.

Thus, Kondratieff is not able to "close the loop" of his explanation except by assigning a particular and major role to agriculture, which on the basis of its nature alone, constrains the development of industry and that of the overall movement of the economy.

In a very surprising way, we thus come back to a traditional question of standard economic thought: <u>in what way is the sphere of agricultural production a fetter on capi-talist development?</u> Kondratieff's conclusion does not relate to the stationary state, but to a development at a particular rhythm. This rhythm of long duration expresses to some extent capitalism's capacity to adapt to the consequences, on the amount and the structure of the accumulation fund available, of the amounts tapped off by property ownership from the income of society. Unlike Ricardo (not to mention Malthus) the limit on economic development is no longer physical (exhaustion of the fertile lands leading to an upward trend in the rent of land), but social, related to the capacity of capitalism to reconstitute this available accumulation fund. In *On the Concepts of the Static, the Dynamic and the Business Cycle in Economics* what Kondratieff objects to in Ricardo and in general in all of the classical school[90] is the failure to study the actual process of change in the elements contributing to the establishment of the natural state (identified by Kondratieff as being one of equilibrium).

In so doing, which was the object of his work, he renders dynamic the conditions for the definition of the equilibrium, and by corollary, as a result of its reversibility, the horizon ceases to be finite[91]. But it must be noted that he can do so only by introducing exogenous elements, and above all by abandoning the embryonic theory of distribution which is found in Ricardo. It would soon be a constant for the theoreticians of the "long cycle" that the dialectic of allocation / distribution should not be linked to the analysis of the long movements of accumulation, a constant that would endure right up to the theory of regulation.

Relatedly, one may note that his explanation is valid only if it is assumed that the relations between the only two sectors referred to by Kondratieff are stable, something that, by hypothesis, cannot be accepted. Indeed, the problems of social organisation related to the circulation of goods or to the mobilisation of the accumula-

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89 Kondratieff is very explicit: "The shift between agriculture and industry, in the downward trends of their costs of production, is the principal cause of the general increase in the purchasing power of agricultural products during the period in question. One can readily understand this delay when one refers to the specific features of the processes of agricultural production, (which is primarily organic), that make them not very suitable for mechanisation, or their marked dependence with respect to space, etc.," Kondratieff 1928b, p. 50 and 1992e, p. 444 (underlined by himself).

90 This passage was not included in the English version of 1925, see Kondratieff 1992a, p. 3.

91 Of Ricardo, he wrote: "He studies the economy as if he supposed that these trends have al-ready reached their conclusion and the elements of economic reality are at a natural level and in a state of equilibrium,", ibid.

tion fund (restricting ourselves to the aspects cited by Kondratieff) will necessarily modify the relative weight and the internal structure of each of the two sectors, but will also generate different activities and forms of production and of social organisation. Thinking that one could replace this analysis founded on the relations between agriculture and industry by a different one in more general terms of conditions of access to raw materials does not change the essence of this criticism. Indeed at this point it sends us back to a question already considered earlier, when we were defining the *essential capital goods*, namely the understanding of the degree of the productive forces which cannot be reduced to sectoral relationships.

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In prison, Kondratieff would continue to work on his project of a historical synthesis of the economic dynamics to render more unified his vision of the alternations between long phases of depression and of expansion, integrating into it different types of cycle.

In a letter to his wife, dated at the end of 1934, he presented his plan like this: "*As soon as I have finished, I shall start a short book on long oscillations: the layout and the content of it are perfectly clear to me. Then, I shall write a book on short cycles and crises. I shall then come back to the introductory part, on general methodology, the out-line for which I have already sent you. Finally I shall finish off the whole thing with a fifth book on the statistical theory of the genetic or the socio-economic development. But, those are projects which demand the strength of serenity and faith, so they may remain at the project stage ...*[92]"

92 Reported by Makachéva, N.A., *Biographical Sketch*, in: Kondratieff, N.D.: *Problems of Economic Dynamics*, Economica, Economic Heritage Collection, Moscow, 1989 (in Russian).

EUROSTAT COLLOQUIUM

HISTORY OF BUSINESS CYCLE ANALYSIS

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TINBERGEN'S BUSINESS CYCLE ANALYSIS

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TABLE OF CONTENTS

1. Abstract

During the 1930s Tinbergen was employed at the Dutch Central Bureau of Statistics and involved in business cycle analysis. He was looking for causal mechanisms producing these cycles and found the a-theoretical approach used there to be inadequate. Therefore, he developed a variety of mathematical schemes that generated wave-like patterns, which could fulfil his specific theoretical and empirical requirements. This case shows that business cycle analysis cannot be a matter of statistics alone but should integrate three different disciplines: economics, statistics and mathematics. This, in fact, was the original research agenda of the Econometric Society.

Each discipline on its own was insufficient to provide a complete explanation of a business cycle. Most economic theories were narratives and therefore did not include the mathematical tools needed to build representations of the cycle mechanism. Moreover, these accounts were largely static, lacking the dynamics to treat cyclic phenomena. Statistical analysis alone can only indicate correlations between economics factors, but can never elucidate causal connections. Whereas, mathematics can be used to make a range of possible dynamical schemes, the results still have to be assessed on their economic meaning and statistical significance.

Econometrics is often seen as a part of statistics. Although mathematicizing economics is seen as a necessary step towards making economics operational, mathematical models are considered to be more or less singular. However, there was no single unique way that mathematics could or should enter economics. Economic reality itself does not prescribe any particular kind of formalism. On the contrary, our understanding of business cycles is determined by the kind of mathematical apparatus we use.

2. Introduction

If two parties should be brought to an agreement, and they have not so much in common – the differences are more eye-catching than the similarities, so they are suspicious toward each other –, then usually an autonomous mediator is called upon. The task of this mediator is to bring both parties – step by step – closer to each other by carefully formalising each result in the negotiations. After both parties have entered into an agreement, the task of the mediator has ended, and if everything has been done well, the agreement should leave the mediator's role indiscernible.

The above metaphor of a mediator, of course, is borrowed from Morgan and Morrison's (1999) account of models as mediators. This metaphor will appear to be very apt to account for the development of Tinbergen's schemes of the business cycle mechanisms in the 1930s. The parties that had to be brought into an agreement were on one side the more verbal accounts of business cycle phenomena and its explanations, and at the other side quantitative economic data describing the cycles and the economies in which they appear. The agreement was drawn up in mathematics; the negotiations were carefully done by consecutively formalising bits of both parties and adding them to already obtained results of agreement. The choice of the mathematical formalisation was very critical in this mediating process, but as soon as the agreement was concluded – Tinbergen's first macroeconometric models – the essential role of mathematics in this process was no longer acknowledged. The paper's aim is to bring back to the foreground the essential role of mathematics in the mediating process of modelling.

Somewhere else (Boumans 1999), I have compared model building with baking a cake without having a recipe. It is a trial and error process by integrating the required ingredients till you have something of your taste.

In Tinbergen's case, the ingredients were theoretical ideas, policy views, mathematisations of the cycle, metaphors and empirical facts. The theoretical ingredients were verbal explanations of (phases of) the cycle and Walrasian mathematical static-equilibrium systems. As a socialist Tinbergen aimed at planning instruments to control the business cycle and was therefore looking for endogenous causal explanations of the cycle. As a physicist he was familiar with the harmonic oscillation described by second order differential equations. And as an employee of Statistics Netherlands (Centraal Bureau voor de Statistiek, hereafter CBS) he had at his disposal the statistics describing several economies and business cycles. The facts of the business cycle phenomenon, the so-called Juglar, were that the cycle period was about 6 to 10 years and that it was a maintained cycle, almost not increasing or decreasing in amplitude. Tinbergen succeeded to integrate all these ingredients into one model and the next sections show how he did it, step by step. Special attention will be paid to the role of mathematics in this model building process. But first a short biography of Tinbergen in this period will be given in the next section.

3. Short Biography of Jan Tinbergen

Jan Tinbergen (1903-1994) studied physics from 1922 to 1926 at the University of Leiden (Netherlands) where Paul Ehrenfest was his most influential teacher. Tinbergen's strong social feelings led him to become an active member of the Social Democratic Labour Party. Within the Labour Party there was a strong antimilitaristic movement. Tinbergen's sympathized with this movement and refused to serve the army as a conscript. As compensation, he had to work in the prison administration in Rotterdam. His father succeeded in arranging a job at the CBS. He worked there the last part of his alternative service from August 1927 till July 1928 at the Business Cycle Bureau.

After finishing this period of alternative military service, Tinbergen worked for a year on his doctoral thesis. His concern for the unemployed made him feel that he could be more useful as an economist than as a physicist. It was to guide Tinbergen away from theoretical physics to mathematical economics that Ehrenfest proposed the subject for his thesis, minimum problems in physics and economics (1929). This subject was specially chosen because of the probable mathematical analogy between the relevant physical problems and certain economic problems[1]. Tinbergen's introduction to economics was mainly through the works of mathematical economists recommended to him by Ehrenfest. These works were by Arthur Bowley, Vilfredo Pareto, Enrico Barone and Charles Roos. In particular, Tinbergen's introduction to the ideas of Pareto and Léon Walras was primarily through *Bowley's Mathematical Groundwork of Economics* (1924). This was a modernized version of the Walras-Pareto system.

After finishing his thesis, Tinbergen returned to the CBS. During his absence, publications on business cycle research had markedly decreased. It was feared that business cycle research would be taken over by private organizations. The CBS asked Tinbergen whether he could continue his business cycle research and he was willing to do so. He stayed till 1945.

In 1930 the Econometric Society was founded in Cleveland, U.S.A., and the first European meeting was to be held in Lausanne, Switzerland, the year after. Tinbergen became charter member and presented a paper in Lausanne. It is remarkable how fast he became one of the leading figures within this Society. But also in The Netherlands his work soon found acknowledgment. In 1931, he became lecturer in statistics at the University of Amsterdam and in 1933 he was appointed Professor Extraordinary of Mathematics and Statistics at the Rotterdam School of Economics.

[1] (Boumans 1993) discusses in more detail Tinbergen's application of physical analogies to economic analysis.

In 1936, Tinbergen was commissioned by the Economic Intelligence Service of the League of Nations to undertake statistical test of the business cycle theories examined for the league by Gottfried von Haberler (1937). He worked at this task for two years and reported his results in two volumes, *Statistical Testing of Business-Cycle Theories*, published in 1939.

4. Critique of Empirical Business Cycle Research

The CBS borrowed its methods from the Harvard Committee on Economic Research and the Berlin Institut für Konjunkturforschung. Business cycle research at these institutes consisted of the construction of so-called "barometers" to forecast business cycles. That is to say, their research concentrated on investigating whether certain economic time-series were correlated. If there is a lag between correlated time-series then it is possible to forecast the course of one time-series with the aid of the other.

The Harvard Committee on Economic Research, under the direction of Charles J. Bullock, Warren M. Persons and William L. Crum, owed its international fame to the barometer based on three indices of the business cycle, the so-called A-B-C curves. These three indices represented "speculation" (A), "business" (B), and "money" (C), and were lagged-correlated. B lagged about six month after A, and C about four month after B. Therefore, A forecasted B and A and B forecasted C.

Tinbergen opposed the non-theoretical character of the Harvard barometer. His very first scientific publication, 'Over de Mathematies-Statistiese Methoden voor Konjunktuuronderzoek' (1927; On Mathematical-Statistical Methods of Business Cycle Research), was a review of business cycle research, which in particular criticized the Harvard approach for not being based on any kind of theory of causation. Moreover, Bullock, Persons and Crum (1927, 79) had emphasized that their method was not based on any theory whatsoever; on the contrary, the curves "derived solely from observation of the facts".

Causal relations have, indeed, received increasing attention from us; but no theory of causation or of time relation between cause and effect ever entered into the construction of the index (idem, 79).

In addition, they observed "how foreign to actual experience are fixed mechanical, or exact mathematical, relationships in the economic world" (idem, 79).

Tinbergen (1927) claimed that the aim of correlation analysis should ultimately be the recovery of causal connections, as Karl G. Karsten had done. Karsten (1926), who touched "not without merit" upon this problem of causal relations (Tinbergen 1927, 718-719), had shown the existence of cumulative relations between the Harvard barometer indices, which he interpreted as causal relationships. In the first place, the cumulative values of the Harvard B-index parallel those of the Harvard A-index, with a lag of three months:

$$\sum_{i=1}^{t} B_i = A_{t+3} \qquad (1)$$

Secondly, the C-index was a cumulative of both the A and B indices:

$$\sum_{i=1}^{t} \left(\tfrac{1}{4} A_i + \tfrac{3}{4} B_i\right) = C_t \qquad (2)$$

Thus, according to Karsten (1926, 417), the B-index was the "generating force" of the three; the other two indices depended upon and were derived from the changes in the business index. Tinbergen found these cumulative relations exemplary for the kind of causal relation one could expect in business cycle research.

Beside the possibility that cumulative relations could lead to causal connections, they had the advantage that they could explain the existence of variable lags. "The quadrature theory [i.e., cumulation theory] is that *the time-lags between the cycles of various economic phenomena are constant functions of the periods of the cycles*" (Karsten 1924, 16). Both cumulative relations, (1) and (2), above are formulated for the discrete case. For the continuous case, cumulations can be expressed by integrals[2]:

$$\int_0^t B(\tau)d\tau = A(t+3) \qquad (3)$$

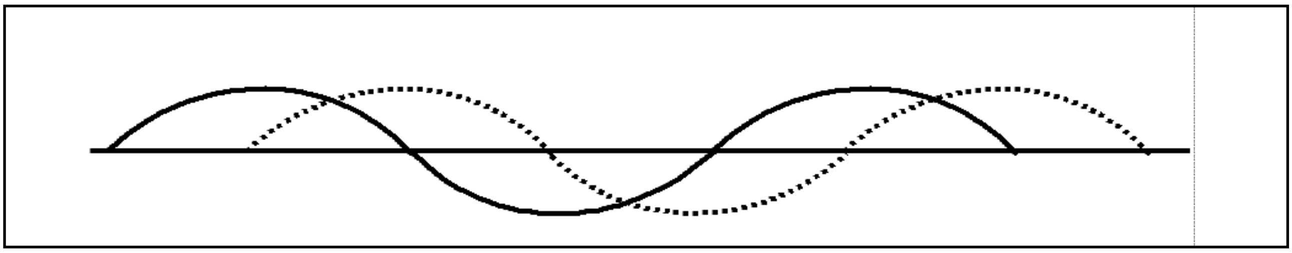
$$\int_0^t \left[\tfrac{1}{4} A(\tau) + \tfrac{3}{4} B(\tau)\right] d\tau = C(t) \qquad (4)$$

But then the cumulative relation between two quantities, *X* and *Y*, can also be represented by a differential equation:

$$\int_0^t X(\tau)d\tau = Y(t) \quad \Rightarrow \quad X(t) = \dot{Y}(t) \qquad (5)$$

The second equation shows that the maxima and minima of one cycle ($\dot{Y}(t) = \frac{dY}{dt} = 0$) coincide with the zero points of the other (*X(t)* = 0), so one can say that one cycle lags a quarter-period behind the other, see Figure 1. If the period of one cycle is not constant neither is the lag.

Figure 1 One cycle is the cumulation of the other



5. Critique of Economic Theory

Tinbergen was looking for causal explanations of the business cycle, but economic theory did not provide the appropriate mechanisms either. On the one hand, business cycles were explained by exogenous influences; on the other hand, each cycle was examined and explained individually or worse still, each phase of a cycle was explained separately. However, there was one exception, Albert Aftalion's (1927) 'Theory of Economic Cycles Based on the Capitalistic Technique of Production'.

An economic dynamics could be constructed based on the [lag] relation between economic quantities, which results in the derivation of perfect cyclic oscillations of an economic system. This is the mathematical interpretation of Aftalion's crisis theory.

2 This explains the name "quadrature theory". Quadrature stands for the process of determining the area of a plane geometric figure by dividing it into a collection of shapes of known area (usually rectangles) and then finding the sum of these areas. The integral denotes this process for infinitesimal rectangles.

I mention this theory in particular because it explains most clearly how the relations considered here can happen, in that every cycle already contains the seed for the next cycle and thus real periodicity occurs (Tinbergen 1927, 715; trans[3]).

Aftalion's thesis was "that the chief responsibility for cyclical fluctuations should be assigned to one of the characteristics of modern industrial technique, namely, the long period required for the production of fixed capital" (Aftalion 1927, 165). To producers the value of their products depends on the price they expect to obtain for them, that is to say, the value depends on the forecast of future prices. Aftalion assumed that the expectations of those directing production are, alternately, too optimistic and too pessimistic. "In other words, the rhythm is a consequence of the long delay which often separates the moment when the production of goods is decided upon and a forecast is made from the moment when the manufacture is terminated, and the forecast is replaced by reality" (idem, 165). Producers forecast future prices on the basis of present prices and the present state of demand. "That is the source of their errors. In modern capitalistic technique the actual state of demand and prices is a bad index of future demand and prices, because of the long interval which separates the moment when new constructions are undertaken from that when they satisfy the demand" (idem, 166).

In a paper "Opmerkingen over Ruilteorie", published in 1928 (Observations on Exchange Theory), Tinbergen constructed a numerical example demonstrating how a delayed adjustment of supply to price would generate fluctuations about equilibrium over time. Shortly after this he stumbled across an empirical example of this numerical construction, a pork market study by Arthur Hanau (1928) (Tinbergen 1928, 548n; see also Magnus and Morgan 1987, 120). Hanau was researcher at the Berlin Institut für Konjunkturforschung run by Ernst Wagemann. According to Tinbergen, this scheme of delayed supply adjustment to price could be extended by taking into account expectations based on observed past fluctuations, or by attributing a delay to demand. "All these assumptions lead to the same kind of results, of which the essence was stated above and consists in the explanation of cyclic motion by the economic mechanism itself" (Tinbergen 1928, 546; trans).

6. Early Business Cycle Schemes

At the first European meeting of the Econometric Society in 1931, Tinbergen (1933a) had several possible mathematical formalizations of an endogenous business cycle mechanism to offer for consideration. Hanau's (1928, 1930) research into the pork market, "le cas le plus simple" (idem, 37), served as point of departure:

Scheme I

Supply: $A_0 + A_1 p(t-\theta)$

Demand: $B_0 - B_1 p(t)$

where A_0, A_1, B_0 and B_1 are positive constants and $p(t)$ the deviation from the equilibrium price P at time t.θ was the time needed to produce the relevant commodity. The mechanism represented by this scheme generated a cycle with a period equal to 2θ. This scheme, better known as the cobweb mechanism because of the similarity of its graphical representation and a cobweb, was the simplest explanation of an economic cycle and a mathematical formulation of Tinbergen's earlier numerical example.

[3] Some of the texts discussed in this paper are written in German or Dutch. When I have translated quotes from these texts, these are indicated by "trans".

However, the aim was to find mechanisms that could explain the so-called Juglars. These were business cycles with a cycle period about 6 to 10 years. Scheme I implied a production time of 3 to 5 years, which is unrealistic for most production processes. To arrive at a more realistic representation of business cycles, Tinbergen examined more complicated schemes to see what influence each "complication" that was introduced could have on the length of the cycle period.

In a second scheme he introduced "demande spéculative". There was some empirical evidence that demand could also be influenced by price changes, for example, in the case of wholesale lumber dealers, or in the case of corn speculation.

Scheme II

Supply: $A_0 + A_1\, p(t-\theta)$

Demand: $B_0 - B_1\, p(t) + B_2\, \dot{p}(t)$

where B_2 is positive and $\dot{p}(t)$ denoting the time differential of price *p, dp(t)/dt,* indicates price changes. Now the period of the solution (T) lies between:
$4/3\ \theta < T < 2\theta$. So, the introduction of a differential shortened the period of the business cycle with respect to the production lag. In other words, if scheme II was considered as a possible explanation for the Juglar, it even assumed a longer production time.

Another way of complicating the scheme was to introduce purchasing power into the demand function. First, he considered constant purchasing power, C.

Scheme III

Supply: $A_0 + A_1\, p(t-\theta)$

Demand: $\dfrac{C}{P + p(t)}$

The solution of this scheme had a period's length equal to 2θ. So, constant purchasing power did not influence the cycle's period. Next, he assumed purchasing power dependent on economic activity, which he defined by the numbers of workers employed during the production process:

$$N(t) = \alpha \int_{t-\theta}^{t} \left[A_0 + A_1 p(\tau)\right] d\tau$$

If the wage is constant and equal to S, then total purchasing power equals SN, and the scheme becomes:

Scheme IV

Supply: $A_0 + A_1\, p(t-\theta)$

Demand: $\dfrac{S\alpha \int_{t-\theta}^{t} \left[A_0 + A_1 p(\tau) d\tau\right]}{P + p(t)}$

The cycle's period was equal to 2.7θ. Thus, by assuming a purchasing power dependent on economic activity, Tinbergen was able to extend the period compared to the production lag, and thus arrived at a more realistic business cycle mechanism.

In 1931 Tinbergen found another empirical example of an endogenous cycle: the shipbuilding cycle. Moreover, a mathematical formulation of its mechanism showed how a lag of two years could generate a cycle of eight years. The shipbuilding market mechanism was a combined lag and cumulative relation:

$$\dot{X}(t) = -aX(t-\theta) \qquad (6)$$

where X represents world tonnage, and θ the average needed time to build a ship, which was approximately 2 years. The parameter *a* was a constant value between ½ and 1. The cycle generated by this mechanism had a period equal to 4θ = 8 years.

7. Synthetic Economics

With the above theoretical and empirical results in mind, Tinbergen gradually developed a larger program for business cycle research to deal with its central question: "is it possible for an economic community to show a swinging movement without the external non-economic factors on which it is based showing such a movement?" (Tinbergen 1933b, 8; trans). The first public event in which he explicated such a program was his inaugural lecture, 'Statistiek en Wiskunde in Dienst van het Konjunktuuronderzoek' (Statistics and Mathematics of Use to Business Cycle Research, 1933b) upon appointment as professor at the Rotterdam School of Economics. This lecture offered a survey of the business cycle research that had already taken place as well as a kind of program, or work proposal, that remained to be done. Other then the above mentioned schemes were possible as candidates for movement generating mechanisms, on the condition that they were dynamic. A scheme was called dynamic when at least one of its equations was dynamic, that is, a relation between variables that relates to different moments of time. Dynamic relations were obtained by introducing lag terms, differentials or integrals.

But these mathematical considerations were just one part of the proposed program. Each candidate scheme, or even each equation in it, had to be statistically verified by regression analysis. Any achieved regression equation was called "analytical knowledge". A "closed" system of regression equations, that is, a system of equations in which the number of variables equals the number of equations was called "synthetic knowledge". The terminology was clearly borrowed from Henry Moore's *Synthetic Economics* (1929).

Moore's "Synthetic Economics" was meant to synthesise two bifurcated mathematical approaches in economics, Walras's "pure" general equilibrium theory and Cournot's statistical approach. However, Walras's equilibrium system was only a static system, while it was Moore's aim to develop a dynamic economics to "give, by means of recent statistical methods, a concrete, practical form to the theoretical ideas of moving equilibria, oscillations, and secular change" (Moore 1929, 4). This "practical form" was to present "all of the interrelated, economic quantities in a synthesis of simultaneous, real equations" (idem, 5).

There are three special characteristics which I should like the name Synthetic Economics to imply: (1) the use of simultaneous equations to express the consensus of exchange, production, capitalization, and distribution; (2) the extension of the use of this mathematical synthesis into economic dynamics where all of the variables in the constituent problems are treated as functions of time; and (3) the still further extension of the synthesis to the point of giving the equations concrete, statistical forms. With these implications Synthetic Economics is both deductive and inductive; dynamic, positive, and concrete (idem, 6).

According to Moore, the "synthetic method" had three advantages. First, it eliminated many controversies in economics as to the causes of phenomena. It showed that each causal relation is only a partial truth; "that the sum of the partial truths is not the whole truth; that the proper weight and place of each partial truth may be specified; and that the ensemble of the determining conditions may be mathematically expressed" (idem, 6-7).

A second advantage was that it indicated precisely when an economic problem was solved. A problem was not only solved when it was a mathematical solution to a system of as many independent equations as there are unknown quantities in the problem, but also the equations themselves had to be empirically derived and, "consequently, that the problem admits of a real solution" (idem, 7).

But, "by far the chief advantage" was that "it gives ground for the hope of introducing into economic life rational forecasting and enlightened control" (idem, 8). To solve the problem of the rational forecasting of oscillations, a complete theory of oscillations could be approached by successive approximations. A first approximation would be to take first into account the most important cause of perturbation, and subsequently combining this with the effects of other perturbing causes (idem, 9).

According to Tinbergen two different kinds of synthetic knowledge were possible:

Either there has to be a certain complex of economic phenomena that, by first approximation, behaves independently of the rest of the economy, and can be lifted out and studied separately, or one has to consider economic society as a whole, which can be done in an approximate or more detailed manner (Tinbergen 1933b, 6; trans.)

An example of an economic phenomena complex, behaving independently of the rest of an economy, was Hanau's investigation of the pork market, his scheme I. The points of departure of the second possibility were his schemes III and IV.

8. Quantitative Business Cycle Theory

In a survey on "quantitative business cycle theory", Tinbergen (1935a) systematically and explicitly outlined his criteria for an appropriate business cycle theory, which for the largest part fitted into Moore's Synthetic Economics program: "The aim of business cycle theory is to explain certain movements of economic variables. Therefore, the basic question to be answered is in what ways movements of variables may be generated" (Tinbergen 1935a, 241). And so, the core of the business cycle theory was the "mechanism", that he defined as "the system of relations existing between the variables; at least one of these relations must be dynamic. This system of relations defines the structure of the economic community to be considered in our theory" (idem, 241-2). But, what Tinbergen more than Moore ever did was emphasizing the distinction between the mathematical form and the economic meaning of the equations.

The mathematical form determines the nature of the possible movements, the economic sense being of no importance here. Thus, two different economic systems obeying, however, the same types of equations may show exactly the same movements. But, it is evident that for all other questions the economic significance of the equations is of first importance and no theory can be accepted whose economic significance is not clear (idem, 242).

Besides the condition that at least one dynamic equation should appear in the mechanism, the other mathematical requirements were that the mechanism should be a "closed" system of equations, that is, a system that contains just as many equations as variables, and "the analytical form of the equations is simplified as much as possible" (idem, 242). One way of gaining simplicity was Frisch's "macrodynamic" approach, that is "the grouping of the elements, which has its statistical counterpart in the calculation of index numbers of all sorts" (idem, 243).

After outlining these criteria for a business cycle theory, Tinbergen discussed "the most important dynamic relations existing in real economic life which may, or must, be chosen as starting points of an adequate business cycle theory", labelled by him as "the facts" (idem, 243). As examples of adequate business cycle theories, Tinbergen mentioned the mathematical theories of Frisch (1933a), Kalecki (1935), Roos (1930) and Vinci (1934). Tinbergen also discussed his own "lag scheme". This five-equation scheme was a generalization of his earlier scheme IV, in which purchasing power was dependent on economic activity.

Mathematical shaping was an essential element of Tinbergen's business cycle research in the 1930s. Economic theory did not provide any guideline that could lead to an appropriate formalism. It was a trial and error process that started with the assumption of a production lag. As was empirically shown by Hanau (1930) and theoretically by Aftalion (1927), lags generate endogenous fluctuations. But to base dynamics on a production lag alone had several disadvantages. In the first place, as discussed above, to explain a Juglar the assumed production time would have to be far too long. Reason for Tinbergen to introduce into the schemes all kinds of complications. In the second place, the disadvantage of postulating lags is that they must be given in advance and have a fixed length. "This has been repeatedly felt as a too rigid representation of reality" (Tinbergen 1933b, 13; trans). However, besides the lag relation other dynamic relations are possible, namely those containing differentials and integrals. From physics, Tinbergen knew that second order differential equations generate cycles. For example, differentiating (with respect to time) an equation in which a differential and an integral term appear lead to the equation of the harmonic oscillator.

$$a\dot{y}(t) + by(t) + c\int_0^t y(\tau)d\tau = 0 \quad \rightarrow \quad a\ddot{y}(t) + b\dot{y}(t) + cy(t) = 0 \qquad (7)$$

An advantage of differential equations is that differentials refer to very small time intervals. $\dot{y}$ = *dy/dt*, where *dt* can be approximated by a very small difference in time Δt. So that:

$$\dot{y} \approx \frac{y(t) - y(t - \Delta t)}{\Delta t} \qquad (8)$$

Considering the shorter time many production processes need nowadays, the appearance of only direct affective causes can be called a realistic feature in view of this. Thus, what really matters is the question just posed: can quantities with an integral character and a differential character, respectively, be found and do these quantities play an important role in the business cycle? (Tinbergen 1933b, 14-15, trans).

At the Leiden meeting of the Econometric Society in 1933, Tinbergen raised this question most explicitly: "Is the theory of harmonic oscillation useful in the study of business cycles?" To deal with this question a special colloquium-lecture on harmonic oscillations by Ehrenfest was planned. Because of his sudden death on September 24, this lecture never took place (Marschak 1934, 187). Tinbergen proposed to start "from the mathematical nature of harmonic oscillations and seeking among the main economic relations those likely to fit into the harmonic pattern" (idem, 188). Accordingly, he marshalled economic relations into two groups: (1) "differential phenomena", mainly functions of the rate of price change, $\dot{p}(t)$, and (2) "integral phenomena", mainly functions of $\int pdt$, where p is again the price. Statistical tests, however, had persuaded him not to give too much credit to most of the hypotheses of group (2), because the correlations he had hitherto found were too small (idem, 188).

In his 1935 survey, Tinbergen discussed this issue again. To make "closer approximations to reality" (1935a, 277), differentials, $\dot{p}(t)$, and integrals, $\int pdt$, were added to the lag schemes. Thus, in general, the reduced form equation of a business cycle scheme would have the following shape[4] :

$$\sum_{1}^{n} a_i p(t-t_i) + \sum_{1}^{n} b_i \dot{p}(t-t_i) + \sum_{1}^{n} c_i \int_{0}^{t-t_i} p(\tau) d\tau = 0 \qquad (9)$$

The requirement was that the parameters satisfy the "wave condition" and the "long wave condition". The "wave condition" indicated that the solution to the above reduced form equation should consist of a sine function, *p(t)* = C $h\tau\sin(wt)$, so that the time shape of *p(t)* is cyclic. The "long wave condition" prescribed that the cycle period should be long compared with the "time units" and that the cycle should not differ "too much from an undamped one" (idem, 280). According to Tinbergen, "These conditions will be a guide in a statistical test of the different schemes as to their accord with reality" (idem, 280). As a first approximation to these conditions, Tinbergen put h = 1 and w = 0. Then the period of the cycle, $2\pi/w$, goes to infinity. A consequence of these conditions was that:

$$\sum_{1}^{n} c_i = 0 \qquad (10)$$

In other words, mechanisms "only then lead to long, not too much damped waves when the integral terms are of small importance" (idem, 281).

Tinbergen also considered a second approximation of the long wave conditions by assuming that $h = 1 + \delta$ and $w = \varepsilon$, where both δ and ε are very small. Again this resulted in restrictions on the parameters of the possible mechanisms. Tinbergen considered several mechanisms as possible explanations of the business cycle. The wave conditions were used to detect the correct mechanism by comparing the order of magnitude required by the conditions with the estimated parameter values. But to find out whether these possible mechanisms "can explain real business cycles and which of them resembles reality" (idem, 281), statistical verification was again the necessary next step in the analysis.

9. Tinbergen's Modelling Program

Tinbergen's research program of the first half of the 1930s can be briefly characterized as a combination of two methods, mathematical shaping and statistical verification. Mathematical shaping generated potential business cycle mechanisms, which had to be verified empirically. However, this typical combination of mathematics and statistics disappeared in the second half of the thirties, when Tinbergen implemented a new program, which products were not called "schemes" but were labelled as "models"[5]. In macroeconometric modelling, which was in fact what this new program was all about, mathematical shaping did not partake anymore.

Tinbergen was the first to succeed in modelling a real economy on the basis of this new program. In 1936 he presented a model of the Dutch economy to the Dutch Society of Economics and Statistics; the very first

[4] There is apparently a misprint in the original text (idem, 279). Above I have reproduced the corrected version.

[5] The very first time Tinbergen used the term "model" was in his 1935 paper 'Quantitative Fragen der Konjunkturpolitik' (Tinbergen 1935b).

macroeconometric model in the history of economics[6]. The paper was read and published in Dutch, but in the same year Tinbergen was commissioned by the League of Nations to undertake statistical tests of the business-cycle theories. The results were published in a two-volume work, *Statistical Testing of Business-Cycle Theories* (1939). The first contained an explanation of a method of econometric testing and a demonstration in three case studies of what could be achieved. The second volume developed a model of the United States; the second macroeconometric model in the history of economics.

Meanwhile, Tinbergen wrote several reports on his work at the League. They provide us of an explicit account of what the early modelling practice embraced. On several occasions, Tinbergen stressed the necessity of simplification.

Mathematical treatment is a powerful tool; it is, however, only applicable if the number of elements in the system is not too large. Subjects, commodities and markets have, therefore, to be combined in large groups, the whole community has to be schematised to a "model" before anything fruitful can be done. This process of schematisation is, of course, more or less arbitrary. It could, of course, be done in a way other than has here been attempted. In a sense this is the "art" of economic research, depending partly on the attitude in which the approach is made. (Tinbergen 1937, 8).

The model was viewed as a system of equations governing the movements of the various elements in economic community. This system consists of "a network of causal relationships", "relationships of definition", and "technical or institutional connections" (idem, 8).

The "method" Tinbergen employed to understand the causation of business-cycle phenomena "essentially starts with *a priori* considerations about what explanatory variables are to be included. This choice must be based on economic theory or common sense" (Tinbergen 1939b, 10). The equations were chosen to be linear, with parameters that remain constant over time. "The use of linear relations means much less loss of generality than is sometimes believed" (idem, 11). The values of the parameters were found by multiple regression analysis. The accuracy of these results was testing by applying "statistical tests of significance". Apart from these statistical tests, "economic tests of significance" were used. "The most important one is that of their algebraic sign, which in most cases the economist knows on a priori grounds" (idem, 13). To "understand the mechanism of business cycles", the aim was to develop an increasing number of relations, "representing the network of causal connections forming the business-cycle mechanism", until a "complete system" of "elementary equations" was obtained (idem, 15). Completeness is achieved when a system has as many relations as there are variables to be explained.

The word "complete" need not to be interpreted in the sense that every detail in the complicated economic organism is described. This would be an impossible task which, moreover, no business-cycle theorist has ever considered as necessary. By increasing or decreasing the number of phenomena, a more refined or a rougher picture or "model" of reality may be obtained; in this respect, the economist is at liberty to exercise his judgement. (Tinbergen 1939b, 15)

The procedure he employed, to statistically test existing business-cycle theories, consisted of two stages: Firstly, the variables that a given theory provides must be tested by multiple regression analysis, and secondly, the system of numerical values found for the causal relations must be tested to see whether it really yields a cyclic movement when used in the reduce form equation. Tinbergen was quite aware of the fact that economists did not agree upon which were the most important causes of the business-cycle phenomenon. From

6 The original paper of 1936 is available in English under the title: 'An Economic Policy for 1936' in (Tinbergen 1956). For a revised version in 1937, which concentrates on econometric aspects, see (Tinbergen 1937).

Ehrenfest he had learned to formulate differences of opinion in a "nobler" way than merely as conflicts. His favourite formulation was cast in the general form: if $a > b$, scholar A is right, but if $a < b$, then scholar B is right. The statement applied to a well-defined problem, and both a and b would generally be sets of values of elements relevant to the problem treated, with possibly a number of components of qualitative nature (Tinbergen 1988, 67).

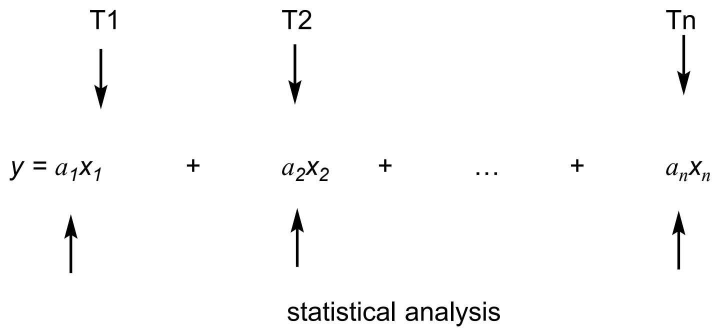
This method was exactly the method he would follow in his work for the League of Nations:

It is rather rare that of two opinions only one is correct, the other wrong. In most cases both form part of the truth … The two opinions, as a rule, do not excluded each other. Then the question arises in what "degree each is correct"; or, how these two opinions have to be "combined" to have the best picture of reality.

[We can] combine these different views, viz. by assuming that the movements … can be explained by some *mathematical function* of all the variables mentioned. We then have not a combination in the physical sense – an addition of two quantities or of two amounts – but a combination of influences. In many cases the mathematical function just mentioned may be approximated by a linear expression (Tinbergen 1936, 2-3)[7].

Tinbergen's method can be presented graphically as follows:

Figure 2



Each model equation had to be assessed in two ways, "deductively" and "inductively". Economic significance was obtained by deducing from economic theories possible causal factors and conditions on the parameter values. Statistical analysis was used to decide which factors were statistically significant and to measure how great their influence was.

Tinbergen's method had become the synthetic method in which mathematical moulding had no part anymore. The mathematical shape of the models was made up of linear difference equations. The differentials and integrals that had played such an important role in earlier mathematical moulding of business cycle schemes were gone. The integrals were omitted because equations containing them were replaced by their differentiated equivalents; see for example equation (5).

The differentials disappeared because Tinbergen had changed his view on the meaning and role of lags in the mathematical relations. In his earlier business cycle schemes, lags had the explicit meaning of production lags and referred to time intervals of about one to two years. One of the main reasons for introducing differentials was that they represented more immediate reactions. But in the later macroeconometric models,

[7] Memorandum on the continuation of the League's business cycle research in a statistical direction (1936, Archive of the League of Nations, Palais de Nation, Genève). I would like to thank Pépin Cabo and Neil de Marchi for bringing this memorandum to my attention.

lags did not have this specific economic meaning anymore; they came to indicate time units of, for example, one month. If time lags are time units, Δt = 1, differentials can be approximated by differences, cf. equation (8):

$$\dot{y} \approx y(t) - y(t-1)$$

10. Conclusions

Tinbergen's method encompassed Moore's synthetic method. But Moore did not discuss the appropriate mathematical shape of a possible business cycle mechanism. As one can see from the above historical account of Tinbergen's business cycle analysis of the 1930s, mathematical moulding played an essential role in the development of business cycle explanations. Tinbergen's business cycle explanations are better considered as exemplars of what could be achieved by the program of the Econometric Society in its early stages. Its scope was "the advancement of economic theory in its relation to statistics and mathematics" and its main object "to promote studies that aim at a unification of the theoretical-quantitative and empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences" (Constitution 1933, 106). One of the founders, Ragnar Frisch, and editor of the society's own journal, *Econometrica*, emphasized this triple approach of econometrics:

Experience has shown that each of these three view-points, that of statistics, economic theory, and mathematics, is a necessary, but not by itself a sufficient, condition for a real understanding of the quantitative relations in modern economic life. It is the unification of all three that is powerful. And it is this unification that constitutes econometrics (Frisch 1933b, 2).

However, in Tinbergen's later modelling practice this founding ideal of the Econometric Society, that is the union of mathematics, economics and statistics, had started to fall to pieces. In the late 1930s, mathematical moulding was the first piece that fell out. But the disintegration would continue in the 1940s, as Mary Morgan has described in her *History of Econometric Ideas* (1990, 264):

Between the 1920s and the 1940s, the tools of mathematics and statistics were indeed used in a productive and complementary union to forge the essential ideas of the econometric approach. But the changing nature of the econometric enterprise in the 1940s caused a return to the division of labour favoured in the late nineteenth century, with mathematical economists working on theory building and econometricians concerned with statistical work.

When mathematical moulding was taken out of the original program an important modelling tool was lost. It is in fact the most effective tool to build the characteristics of the phenomenon to be measured, described or explained into a scheme or model[8]. The various characteristics of a business cycle, or of its generating mechanism, were built into a scheme by shaping the mathematical equations of the business cycle mechanism. Whether differentials or integrals should be added was a part of the discussion of how to mould to scheme such that it incorporated the right cause-and-effect relations: immediate reactions, variable time lags between influences, small production intervals generating large cycle periods, etc.

[8] In my (1999) 'Built-in Justification' paper, I give a more general account of the role of mathematical shaping in the modeling process and apply this to three cases of macroeconomic modeling. Two of them took place in the same period as Tinbergen's work described here and were closely related it: (Frisch 1933) and (Kalecki 1935).

In classical mechanics, there is a close connection between the Calculus of Variations and cause-and-effect relations. It is because of this connection that Karsten wanted to apply the "theory of quadrature" to investigate the kind of relations that exists between economic quantities.

In the calculus such relations are familiar in the form of integrals and derivatives, and although these functions are purely mathematical, they are useful to describe the behavior of related forces in the physical sciences. It is the quadrature theory that economic data or statistics betray the same relationships when similarly treated, and that when this is the case, the economic forces or phenomena measured by statistics may be said to be in quadrature and a real relation is strongly suggested (Karsten 1924, 14).

And it was this connection that made Karsten's approach appealing to Tinbergen.

However, the nature of economic data is totally different from data gathered in physics, that is, passive gained data only available in weekly, monthly, or other regular time-interval versus data obtained in a laboratory under ceteris paribus conditions. Therefore, the mathematical tools to deal with these different kinds of data will be different. Difference equations are simply more suitable to deal with economic data than differential equations. But this observation is only one step in mathematical moulding. What still need to be done are decisions upon the shape of the density functions, the metrics to be used, and the range of the parameter values.

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EUROSTAT COLLOQUIUM

HISTORY OF BUSINESS CYCLE ANALYSIS

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BUSINESS CYCLE ANALYSIS IN NORWAY UNTIL THE 1950s

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TABLE OF CONTENTS

"That 'good times' follow 'bad times' and 'bad times' again after 'good times' are something most people have a vague understanding of. But not so many have the full understanding of how regular and continual this changing up- and downward movement in economic life is, and even fewer have the right understanding of how the bad times naturally and necessarily are developed from the good times, just like rain follows and always must follow sunshine and sunshine follows rain."

This is how the Norwegian economist Einar Einarsen (1868-1913) introduced his book *Gode og daarlige tider* (Good and bad times) from 1904. Einarsen's book, which earned him a position as professor at the University of Copenhagen (Hanisch and Berg 1984: 76), was the first empirical and theoretical investigation of business cycles in Norway. This book reflected the emerging interest in this field in a number of countries. Also older studies from Norwegian economists had been interested in the causes of instability in the economic system. They were, however, occupied with descriptions and explanations of economic crises. As most 19th century economists, they did not seem to recognize "cycles" as a central concept for economic analysis (Schumpeter 1954, Morgan 1990). This is particularly evident in the elderly Professor Aschehoug's (1822-1909) three-volume treatise of modern economics, published in the same decade as Einarsen's book. The volume examining these topics was introduced with a section on "Changing good and bad times: The history of crises", and the concluding chapter is rightfully titled "The theory of crises" (Aschehoug 1908).

Einarsen obviously drew heavily on Clément Juglar's analyses of economic cycles; his comments on other theorists also seems to have been inspired by judgments made in Juglar's main study[1]. In the concluding section he firstly rejects theories focusing on crises and explaining them by "accidental causes". He then moves on to general explanations of the cycles, mainly Jevons' sunspot theory. This was "a great theory from a man of genius", but, like Juglar, he stated that Jevons' theory was not in line with statistical evidence, and that a theory of crises or cycles had to be rooted in the social and economic structure of society. Finally, Einarsen arrived at "the true cause of the alternating good and bad times", found in the relation between credits, prices and the volume of production in the economy. As Juglar, he found that the downturn was caused by factors inherent in the upturn, and vice versa. The cycles were inevitable, like rain and sunshine, though they did not necessarily appear with a regular periodicity.

Einarsen methodology was quite simple; his numbers were reproduced directly from the State's official statistics without any calculations. Thus he ended up with long columns of absolute numbers, where it was (and is) extremely hard for the reader to identify trends and cyclical movements. And even if Einarsen's study was in line with the newer international trends in economic investigation – or maybe just for that reason – its theoretical approach seems to have had a limited influence on business cycle analyses from the early 1920s, when this field of study expanded in Norway. The empirical business cycle analysis of the 1920s and '30s lost interest in the kind of general theorizing Einarsen was occupied with, where causes and effects were discussed in relations to a few variables measuring the state of the national economy. This is a part of an international tendency, which in Norway was strengthened as Norwegian economists and statisticians increasingly came to see business cycles in the domestic economy as a consequence of developments in larger countries or in trade patterns. – This is particularly evident for the late 1920s and 1930s.

[1] Einarsen's references are to the second edition of Juglars *Des Crises Commerciales et de leur retour périodique en France, en Angleterre et aux États-Unis*. Paris: Guillaumin 1889.

1. The economic barometer project in the 1920s – beliefs and doubts

The large economic fluctuations during and shortly after the First World War came to promote an interest for short-term economic statistics. This interest increased after the construction of the Harvard barometer, which received considerable attention also in the Scandinavian countries.

The Swedish short-term statistics and analysis of business cycles expanded rapidly in the 1920s. The key figure behind this expansion was the economist Johan Åkerman, who developed a Harvard-inspired "barometer" that was published regularly from 1922. In Norway, the ambition seems to have been the same as in Sweden in the early 1920s. The immediate goal was to develop a barometer that could give simple prognosis for the economic development, especially early warnings of economic downturns. This became even more important as the general view was that an immanent socialist threat from the radicalised labour movement would be strongly reinforced in times of crises and unemployment.

Just before the war, two commercial magazines in Norway had started to construct and publish each their wholesale price index, and newspapers and magazines constantly brought forward informal prognoses for the state of the economy. From 1922, The Central Bureau of Statistics presented monthly a number of short-term statistics in what the statisticians labelled a "business cycle table". In this table, unemployment among trade union members, interest rates, volume of credits given from private banks, some figures for foreign trade, bankruptcies, the stock market index and a few other time series were presented. Annually, the tables were supplied with long verbal comments from the Bureau economists, and normally supplied with comparative figures from larger countries (USA, Great Britain, Germany and France). The Bureau's plan in 1922 was to "make a thorough analysis of each column, to determine their symptomatic value as a measurement of the economic cycles and thereby approach the construction of a true barometer of crisis" (CBS 1922: 49).

The next autumn, the Bureau started to print graphs illustrating the cyclical movement of some of these series. In an article in 1924, this move was presented as the definite step in the making of a "business cycle barometer". Norway was now, together with Great Britain and Sweden, among the countries that had barometers of the Harvard type. The author of this article, Ingvar Wedervang, explained that each series did nothing else than describe the development. However, "by following the series reciprocal [innbyrdes] movements through earlier cycles one can find out in what order the leading economic events usually happen" (Wedervang 1924). In the 1923-graphs, efforts were made to remove the long-term trend and seasonal movement, so that the cyclical movements should appear more clearly. The person behind these technical operations was probably a young economist, Charles W. Røgeberg. Røgeberg graduated from Oslo University in 1917 and worked in the Bureau for two years. From 1919 until 1921 he went to Harvard to study economics and statistics, and in these two years he had a temporarily employment in the Harvard Committee on Economic Research. After having returned to Norway, Røgeberg worked two years for a larger commercial bank before he re-entered the Bureau in 1923.

From this time on, one would have expected that the Norwegian "barometer" would have been further elaborated and possibly strengthened with new statistical series. But the opposite happened. The short-term analyses of time series were considerably reduced in 1925, and in 1926 it was stated from the Bureau that the production of an economic "barometer" was no longer on their agenda (CBS 1926: 251). The reason for this was said to be that several of the statistical series describing the Norwegian economy covered to short periods of time; hence they could not serve this purpose. An explanation like this was consistent with the already presented idea that the graphs only could be used in constructing a barometer when historical patterns were revealed. Still, the time series had not become shorter from 1923 to 1926 – it seems that some kind of overall pessimism with regard to the barometer-project had occurred.

This is indicated also by the annual business cycle analysis. In 1927, the first year after the denouncement of the barometer-idea, the graphical representation was still made like the Harvard-lines. They were drawn together in the same picture, so that cyclical correlations or systematic time lags could appear as clearly as possible. But the reader was explicitly informed that the lines could not "be used as a barometer for future development" (CBS 1928: 20). And from the next year and throughout the 1930s, the curves were separated and presented either in different frames or above each other in one large figure. Adrienne van den Bogaard (1999: 119) has mentioned this difference between the Dutch barometer of the late 1920s and Harvard-original: The Dutch economists choose principally the same solution as Norwegians after 1927, and v.d. Bogaard sees this separation of curves as an indication of Dutch doubts on the philosophy of the barometer. In Norway, this relation between belief and doubt on the one hand, and integrated and separate graphs on the other, seems to be even more obvious.

2. The empiricist's approach

So why was the barometer-project left – and for what? Historically, the barometer construction has been seen as a part of a more analytical-descriptive program, which was succeeded by a macroeconomic research program with Ragnar Frisch as one of the protagonists. The redirection in 1926 was not inspired by a more theoretical approach, its time was still to come; it was relieved by a more analytical and in some respects less theoretical approaches to business cycle studies.

Ingvar Wedervang, who had worked in the Bureau with questions related to business cycle studies, moved on to a position at Oslo University in 1924 – in 1931, he founded the Institute of Economics together with Ragnar Frisch. The already mentioned Charles Røgeberg kept working in the Bureau. (When one of the authors of this paper stated to work in the Bureau in 1965 Røgeberg was still in position. But he does not seem to have published anything in the preceding 40 years since he wrote his article on how to remove seasonal cycles from foreign trade statistics in 1924.)

The person who came to dominate the business cycle research in the Bureau was Eilif Gjermoe (1887-1980), educated in law and economics and employed in the Bureau from 1917. From 1926 Gjermoe was appointed as the head of one of four departments in the Bureau. This was a very large department, dealing with population statistics, wages, prices, unemployment and a number of other statistical areas. From the appointment of Gjermoe and until the mid 1930s, this department coordinated the presentation of short-term economic statistics. Gjermoe seems to have used the term "barometer" only a few times in 1927/28, and that is when he – as quoted above explained that the graphs should not be interpreted as one.

Gjermoe had soon got interested in time-series studies. From 1917 he published a number of articles examining property prices (1917), bankruptcies (1922), quantity of money (1923), the relations between marriages and selected economic variables (1924), and between crime rates and business cycles (1927). His analysis of marriages and business cycles had striking similarities to, and were probably inspired by, earlier studies from e.g. Arthur Bowley and Reginald Hooker (Klein 1997: 232 f.). Common for all Gjermoe's early investigations were that they directed against areas were relatively long time series were available. His investigations were methodologically motivated, he was fond of lalgebraic exercises, and he looked for – and often found – evidence suggesting cyclical correspondence in his time series.

In contrast to the studies carried out in the start of his career, later studies showed an increasing scepticism to general statements about social and economic regularities and basic concepts used in cycle analyses. While e.g. Ragnar Frisch were among those who maintained the existence of cyclical regularities, Gjermoe flatly rejected theories claiming that the business cycles followed a regular pattern: "It is completely unreasonable that the interplay of different forces in economic life *by themselves* should create a system of regular cycles. If one should suppose that, one also has to suppose that the actual cycle is irregular because the number of different external factors influencing the development must remove any regularity in the movements. It is easier to accept that a sufficiently strong external factor that gives the economy a 'shock' can make the economic activity move in cycles that gradually disappears. When these shocks have different impacts and occur with different intervals, one can also believe that they can create shorter cycles on top of longer cycles. But it is difficult to accept that these shocks, which presumably must be independent of each other, can create shorter or longer cycles where each one has a *specific* duration."

For Gjermoes role as a key operator in the Central Bureau of Statistics, his views on concepts and methods in empirical cycle studies in other statistical institutes are perhaps more interesting. Normal procedure in business cycle studies was to try to distinguish three types of fluctuations: the long-term (secular) trend, the cyclical movement, and the seasonal trend. Normally, the cycles were constructed by identifying and removing the long-term tendency and seasonal trend from the statistical series. Gjermoe had carried out these kinds of operations himself in his early studies. In the late twenties and early thirties he made no studies of this kind but in several purely methodologically directed articles he commented on and partly criticised this starting point. Gjermoe had always been reluctant to accept the underlying economic explanation of what the long-term trend consisted of. In 1923 he quoted Warren Person's explanation that this trend was "the growth element due to the increase of population and development of industry" (Gjermoe 1923: 90). This growth theory – or its backbone, to quote Schumpeter – was not accepted by Gjermoe. He chose to draw a "normal curve" that in sum left half the cyclical curves over the "normal curve" and half the cycles below, but without giving the "normal" any economic interpretation. He was also critical to common methods in the separation of "seasons" and "cycles". There was no reason to believe that the seasonal movement was independent of the business cycle (its strength and composition), Gjermoe explained to his readers, still standard estimation techniques relied on these assumptions[2]. – These and other objections didn't in any way make him leave these concepts but he wrote a number of articles were he suggested different alteration and modifications in estimation techniques (Gjermoe 1928, 1929a, 1929b, 1930).

The interwar Bureau's and Gjermoe's most original efforts came in the business cycle analysis of different industrial branches in the 1930s. Several large in-depth studies were made, probably inspired by the effects of the depression on the Norwegian economy, and made possible by the considerable expansion in the statistics produced from the late 1920s. A quite detailed annual statistics covering manufacturing industries was made from 1927, and in the early 1930 a number of new short-term series were introduced, making it possible to construct a production index for manufacturing industry from 1933. Between 1935 and 1940 Gjermoe published three larger and two smaller books on the development of the manufacturing industry in Norway in "the post-war period", all written in Norwegian (Gjermoe 1935, 1936, 1937, 1938, 1940). Also in these books he tried to register "normal curves" and cycles of different length, but now a number new methods were introduced. He divided the manufacturing industry into 27 groups, – these were not the groups used in the Bureaus own statistics, the classifications were made to suit his analytical purposes. For each group he sampled a large numbers of firms, trying to make the samples as representative as possible, and finally he estimated Cobb-Douglas production functions by ordinary least square methods for all the groups. His sources in the estimation of the parameters were the firms tax assessments and the statistical information reported to the Bureau.

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2 The criticism was directed against Warren Person's and Wesley Mitchell's writings, which were aware of the problem but (according to Gjermoe) still disregarding it in empirical analysis.

The production functions made from the samples were used as a supplement to make reliable time series where existing statistics was weak. For the most "traditional" part of the business cycle study, Gjermoe focused his interest on determining turning points in the time series and identifying "leading" and "lagging" sub-sectors both in the upturns and the downturns. Through these studies, he seems to have gained a new optimism with regard to the possibility of identifying regularities that could be used in prognostic statements[3]. However, the production functions were developed primarily for the study of the interplay of productivity, profits and wages during the cyclical movements. For Gjermoe, a political radical, it was central issue also to find out how incomes, or rather "burdens", were distributed between capital and labour in the times of crises.

From the practitioner we will now turn to the theorist. Gjermoe methodology was fruits of wide readings in Anglo-american, Scandinavian, German and French literature. However, in both his inter-war and rather unknown post-war writings, his more famous contemporary countryman Ragnar Frisch was completely absent from his references, as was Frisch's students and younger colleagues. This was a mutual relationship; when the new "macroeconomics" was constituted after the war and Frisch's students rapidly came to fill the leading positions in institutions for economic analysis and politics, Gjermoe disappeared completely. We will return to Gjermoe and the destiny of his program after having presented the other business cycle research programme in Norway in the 1930s.

3. Ragnar Frisch's approach to business cycle analysis

Business cycle analysis was one of Ragnar Frisch's major projects, if not the major project throughout his most creative and productive period. This note does not aim at more than providing some remarks on the efforts of Ragnar Frisch in business cycle analysis, especially towards indicating the overall scope of his approach, as seen from his own viewpoint, and the elements in his research strategy. The remarks are a mixture of biographic, bibliographic and other observations, some of which have been gleaned from Frisch's correspondance and archival material.

Frisch's business cycle approach has been discussed by several authors, especially by Mary Morgan in the widely read Morgan (1990) and by Jens Chr. Andvig, perhaps the foremost expert on Frisch's interwar macro-economic work, in Andvig (1986). Frisch's famous Cassel essay from 1933 figures prominently in the history of business cycle analysis (as well as in the history of macroeconometric model-building, see Bodkin, Klein & Marwah, 1991), and is discussed by e.g. Zambelli (1992), Louçã (1997), Klein (1998), and Thalberg (1998)[4].

Frisch's publications in English from his project are primarily Frisch (1927, 1928, 1931a, 1933), also Frisch (1938) must to be included. He mentioned or touched upon his business cycle ideas also in various other publications. In addition there are various publications and documents in Norwegian.

Frisch's business cycle project can hardly be deemed a successful one, as he never finalized it and left it largely unpublished. He kept promising a major publication that never appeared[5]. This was not unusual for Frisch, he also announced other publications that never appeared or were published much later. But with regard to his business cycle project these repeated announcements (followed by unannounced withdrawals)

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[3] Here I try to read between the lines, Gjermoe is never explicit about this. But says several places that the study of regularities of leading and lagging sectors is vital for these analyses.

[4] It was also the paper particularly pointed to by the Royal Swedish Academy of Science when it honored Frisch by awarding the first Nobel Prize in economics to him in 1969.

[5] This promise can be found e.g. in Frisch (1928, p.220), (1931a, p.76, n.1), (1933, p.29), (1938, p.3, n.2), as well as in other publications such as e.g. Frisch (1934a, p.271, n.3).

perhaps indicates more the effort and priority Frisch gave to this project, and the urgency he felt to come out with results[6]. The publication he promised, was the more complete solution he claimed to have found, to the problem that Slutsky posed in his 1927 article, rather than a comprehensive publication on business cycle analysis.

Then why did he not deliver? The answer to this question is presumably that he either was not sufficiently happy with the results, or his research strategy simply did not work out. As we shall see, Frisch's approach required a large number of numerical simulations, something at which he was extremely adept, but the problems may have turned out to be more demanding than his after all primitive equipment could cope with.

An eyewitness has confirmed that Frisch reworked his business cycle project as late as the winter 1939/40[7]. Post-World War II he did not touch it again, but by the outbreak of the war it was already obsolete. Jan Tinbergen had initiated the macroeconometric approach. The publication of J. M. Keynes' *General Theory* with its strong appeal to macroeconomic policy-making did not leave much of a role for non-policy oriented business cycle approaches. Hence, Frisch's project disappeared quickly from the scene. Because of the incompleteness of Frisch's publication of his ideas, the discussions of his approach in the literature do not fully do justice to his efforts and ambitions.

After some biographical background and brief remarks on Frisch's views on scientific methods in economics we shall let a conference presentation Frisch gave (in Norwegian) in 1931 on the methodology of business cycle analysis play a central role and serve as a focal point for discussing his work and some of the interactions he had with other practicioners.

Ragnar Frisch (1895-1973) was the jeweller's son who chose to follow a calling to become an econometrician. After studying economics alongside his apprenticeship with a silversmith, Frisch continued his economic and statistical studies after graduation and accomplishment of his journeyman's probation work in 1920. After studying in Paris 1921-23, he completed his doctoral thesis at the end of 1926[8]. For 1927 he had received a Rockefeller fellowship for studies in the United States, a stay which brought him in contact with i.a. Wesley Mitchell, Irving Fisher, and Joseph Schumpeter.

Frisch (1927), written down during the first couple of months during his stay in the United States, was a long essay criticizing the current periodogram techniques of analysing historical time series with regard to finding cycles and proposing new methods that allowed for "changing harmonics". It was through the help of Wesley Mitchell distributed by Rockefeller Foundation. And thus, though it was not properly published, it reached many of the practioners in the field and also found its way to university and research institute libraries. In the essay Frisch mentioned his inability in Oslo "without organized assistance for tabulating and computing work" (p.8) to test his proposed methods as far as he had wished, and he expressed the hope that "some well equipped American research bureau will find it worth while to undertake a thorough numerical test of the methods"[9]. Frisch (1928) was a briefer journal presentation of essentially the same methods.

6 The publication was promised successively as an unspecified monograph, a publication from his Institute of Economics, then an Econometrica article in 1933, and finally a Cowles Commission Monograph in 1934. The Econometrica article seems to have been dropped after Edwin B. Wilson, refereeing it, called it mathematics of little interests to economists. The planned Cowles Commission Monograph were to be called Changing Harmonics: A Study Of The Effects Of Linear Operations Performed On Erratic Shocks.

7 Petter Jakob Bjerve, as told to Olav Bjerkholt.

8 His dissertation was titled: Sur les semi-invariants et moments employés dans l'étude des distributions statistiques.

9 Frisch (1927) is disucced at some length in Morgan (1990, 83-90).

In the spring of 1928 Frisch's father died with the jeweller's business in a vulnerable financial state. Frisch as the only heir, was at a point of no return. A generous invitation from Irving Fisher at Yale University was decisive. Frisch accepted the invitation, put down his silversmith tools for the last time, and took off for United States in January 1930. The visit to the United States lasted one year and a half. During the visit he took prominently part in founding The Econometric Society in December 1930.

Frisch was as a scientist very much a "methods man". When as a young student he surveyed economics, he found it lacking in scientific rigour in several respects. In drafting the constitution of The Econometric Society, Frisch proposed the formulation found until the 1990s in every issue of Econometrica: "… to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences." This terminology was adapted from and practically identical to what he had used to formulate his own research program around the mid-1920s.

> As the quoted formulation hinted at Frisch held strong methodological views on the role of theory in empirical work. He must certainly be counted as a strong proponent of empirical studies, but he had nothing but scorn for empirical analysis not appropriately founded on a theoretical paradigm. He expressed this view many times, e.g. in a panel debate on the status and prospect for quantitative economics at the AEA meeting in 1927, where he complained about the widespread tendency to take 'economic statistics' as synonymous with 'quantitative economics'. The latter term according to Frisch had to do with 'the logic of our quantitative notions'[10].

He vented his view on this in an eloquent and very Frischian way also in his opposition to Johan Åkerman's doctoral dissertation in 1929[11]. Referring to the distinction he had drawn in the above mentioned debate he spoke scorningly about the group of "*geschäftige* statistics compilers and correlation calculators":

> 'Let the facts speak for themselves' has become quite a slogan for this group. It is a dangerous slogan, prone to lead astray those who think superficially and without independence of mind. It is true that there can be cases where we in advance do not have a definite hypothesis, and where it therefore can be useful to play around with the observation material in the hope that this will give us an idea. But this playing around can never become more than a preliminary step. The fundamental contribution to the understanding of the phenomena comes from another part in the analysis.

Both natural and social phenomena adhere to a peculiar and particular advertisement policy. Often they present with trumpets and drums as big numbers a lot of things that cannot avoid attracting the attention of an observer with no preconceptions, but which are utterly unimportant with regard to a deeper understanding of the phenomena. 'Facts that speak for themselves' will often speak in a childish tone. <u>And that is especially true with regard to those phenomena which cover really scientific problems</u>. Where the explanation is obvious, no scientific investigation is needed. And for phenomena where the explanation is not obvious, it turns out again and again the the key to the phenomena is an idea which is <u>not</u> in the observation material, but on the contrary is added to it as a heroic assumption that transcends what has been observed. It is just in such heroic asumptions that the great constructive thinker differs from the average scientific worker.

[10] Mills et al. (1928). Frisch took part as a discussant from the floor and the quote is from the statement he prepared for the proceedings, as requested by Frederick Mills, but it was not printed after all. The panel debate may have been a follow-up of Wesley Mitchell's Presidential Address in 1924, Mitchell (1925).

[11] Frisch (1931c). His rhetorical statements were not meant as criticism of Åkerman.

> This is true both for natural and social sciences. The observation material is and remain a dead mass until animated by a constructive theoretical conjecture. It is the synthesis between theory and observation that gives fundamental contributions to a real understanding of the phenomena.

Although no economics or econometric topic seems to have been out of range of Frischs' interest at this time, his major fields of research effort were at this time "marginal utility analysis", "productivity theory", and "business cycles". In each of these he aimed at giving demonstrations of what the "unification of the theoretical-quantitative and the empirical-quantitative approach" meant. In both of the micro fields he had prepared monograph length elaborations of the theory. In marginal utility analysis he pioneered an axiomatic approach and applied the theory in various directions, e.g. to labour supply, taxation, price indices, in addition to his favourite topic of estimating the income flexibility of marginal utility. In production theory he had worked out the implications of the basic assumptions underlying the multi-factor production function in a very detailed way, clearly preparing to apply it to different kinds of problems, which he indeed did during the 1930s, applications comprised engineering production studies, the diet problem and demography.

The applications he pursued in the two micro fields were both static and dynamic, the business cycle analysis, on the other hand, was *macrodynamic* by definition. The business cycle analysis differed from the other fields, as there was not much to draw on as a theoretical paradigm as the background for empirical studies. The theories that had been launched were conflicting, and as Frisch pointed out, hardly any of them could be said to be a determinate theory. Frisch's approach to business cycle analysis can be interpreted on this background as a search for a confrontation between theory and reality without postulating a specific macroeconomic model. How then to get at the cyclical character of economic variables? Frisch's use of the Slutsky effect, in fact, a reinterpretation of Slutsky's work seemed in this setting a brilliant idea.

Thus what is strikingly missing in Frisch's massive work in business cycles analysis is the search for a macroeconomic paradigm, a set of conjectured macrodynamic equations. A model is found in the Cassel essay and it came to be interpreted as Frisch's theory of the business cycles. But the Cassel essay needs a close reading to verify whether the model presented there was meant as a specific business cycle theory or just an exemplification of some kind. Frisch had from much earlier held the doubts that he finally let out in his 1938 criticism of Tinbergen, that simultaneity implied severe restrictions on the possibility of verifying the autonomous relationships representing the functioning the economy even if they existed. The ultimate purpose of business cycle analysis, as for all Frisch's research, was to provide socially useful information, which for the business cycle analysis meant forecasts. But how could that be offered in a scientific way without an explicit estimated or calibrated macroeconomic model? Frisch had a solution, but it it turned out to be a remote one.

In the United States Frisch had gathered, primarily through his lectures at Yale and in Minnesota, a number of pupils of his approach to time series analysis. They were graduate students, doctoral students and the occasional professor including – Joseph Schumpeter.

Just before Frisch returned to Oslo from USA he had been awarded a chair as professor at the University of Oslo by a special act of the Storting (parliament), which had been notified that Frisch had a pending offer from Yale University for a permanent professorship and ample research facilities. He at the same time held high hopes for getting support from Rockefeller Foundation for an econometric laboratory in Oslo, not least for business cycle analysis.

On his return in June 1931 he went almost directly to Stockholm to take part in the Nordic Economic Meeting[12]. The meeting took place 15-17 June 1931, and Frisch's contribution was titled: "Business cycles as a statistical and theoretical problem"[13]. He aimed in the paper at presenting a general survey of the nature of the business cycle problem and of the methods, economic-theoretical and statistical, by which it could be attacked. Frisch had prepared a synoptic note in which he very briefly stated (with slight reference to W. Wilson) 14 points on the issue and then spoke to elaborate on each one of them.

The points were brief, not particularly systematic, and some of them even cryptic, for those who did not know Frisch's terminology (and who would?). Probably the handout was written at short notice. Frisch had unusual credentials for attacking the business cycle problem in his combination of economic and statistical knowledge, his mathematical abilities and his almost passionate skills in numerical analysis.The points are worth considering as there had never until then been stated anything that came close in comprehensiveness and analytic depth about constructing business cycle models.

The 14 points in the synoptic note were the following:

Economic-theoretical part

1. The connection between the economic-theoretical and the statistical parts of the problem. Necessary for the business cycle theoretician to handle modern statistical tools.
2. Cycles of different kinds: shortrun cycles, longrun cycles, etc.
3. Free and bound oscillations.
4. Impulse problems and propagation problems with a free oscillation. Perturbations and half-free oscillations.
5. The business cycle must for the most part be dealt with as a free oscillation.
6. The business cycle theory (understood as a theory for a free oscillation) must be determined. It must contain exactly the same number of conditions as variables.
7. None of the until now proposed business cycle theories have been determined.
8. In a determined business cycle theory at least one of the conditions must be dynamic. From this follows inter alia that a system of Walrasian equations never can lead to a business cycle theory. A dynamic condition is a condition which connects the values of a certain variable at two (or more) points in time.
9. A complete business cycle theory comprises three problems:

   a) The specification problem: The specification of the relevant variables.

   b) The determination problem: Analysis of the number and the independence of the posed conditions and comparison with the number of variables.

[12] The Nordic Economic Meetings were joint meetings at four-year intervals of the national political economy associations in Sweden, Denmark, Norway and Finland, normally with one speaker from each country. The senior of the two professors in economics, Oskar Jæger, had urged upon Frisch, while in the United States, to provide the Norwegian contribution. Frisch yielded, but found it "very inconvenient". The reason was a forthcoming meeting in Oslo with representatives of the Rockefeller Foundation immediately after the Stockholm event, to discuss the proposal of establishing the Institute of Economics, an issue of absolute concern to Frisch, see Bjerkholt (2000c).

[13] The source for Frisch's presentation is the official proceedings, see Frisch (1931b). The proceedings volume from the meeting contains his introductory speech, with the handout appended. (The handout and the quotes from Frisch's presentation translated by O. Bjerkholt.)

c) The shape problem: Clarify that the posed conditions really lead to cyclical movements. This depends upon not only which variables enter which condition(s), but also of the numerical relationship. Certain values of the numerical parameters characterizing the conditions will may lead to cyclical movements, while other values of the same parameters do not lead to cyclical movements. The numerical character of the conditions is thus essential.

10. An attempt at erecting a determined scheme for a business cycle theory [a model sketch was included in the synoptic note, but omitted here].

Statistical part

11. The decomposition problem for statistical time series.
12. The distinction between prim-relations and confluent relations (phase-relations). Inflated and deflated phase-relations.
13. Pitfalls to watch out for when trying to determine the numerical character of the economic-theoretical laws by means of statistical data. In principle the phase-relations can always be determined, but the prim-relations can only be determined in certain cases.
14. Conditions under which it is possible to determine prim-relations statistically. Even if prim-relations cannot be determined statistically, there is a loophole: a systematic "interview" method which very likely will give useful results.

In his presentation Frisch referred to the points in the handout as a research programme or "lines of advance" in business cycle analysis, necessary to achieve more fruitful and above all more definite results than achieved through the many different and partly contradictory business cycle theories. He emphasized (point 1) the role of statistics as no longer being just a descriptive tool, but also an important element in the explanation of economic waves. This called for the business cycle theoretician to be appropriately equipped with statistical knowledge. He asserted that within 10-15 years it would be an absolute necessity for the business cycle theoretician to possess a wide range of statistical-technical tools.

He reiterated some of his favourite scientific maxims, such as the need to get – as in physics – the theoretical concepts from the observation technique itself, i.e. to define the concepts such that they can be observed empirically through statistics or in other ways. Another maxim was the indispensable need for business cycle mechanisms to be analysed quantitatively, the essential mechanisms could not be arrived at through a purely qualitative analysis, as they typically would be contingent on the quantitative relationship between the magnitudes of key parameters.

The key issues of the "economic-theoretical part" was Frisch's tenets for a business cycle theory. It had to be *determinate* (point 6), it had to be *dynamic* in the Frischian sense (point 8), and it ought to conceive the cycle as a *free oscillation* (point 5).

Frisch also went over more familiar ground to the benefit of the assembled Nordic economists and expressed his concern (point 2) that one had to be prepared for at the outset of analysis that the business cycle that came to the fore after eliminating seasonal movements as well as the underlying trend, could consist of cycles of different length, and interference between different waves might very well make the business cycle look very irregular. This was, indeed, Frisch's home ground, he had conducted countless numerical simulations while in the United States and posed as a test to students in exams he gave, to retrieve the original cycles in time series of constructed data of superposed cycles and shocks.

Frisch explained the difference between free and bound oscillations (point 3), which was a distinction relative to the model or system of equations representing the cycle mechanism. He spoke at some length to elaborate using examples from astronomy, locust invasions, meteorology, Jevons' sun spot theory, Moore's Venus theory, and Beveridge's wheat prices to make his point.

The key terms from the title of the Cassel essay, not yet written, appeared (point 4), clearly referring to the use that Frisch would make of Slutsky (1927) in his modelling of Wicksell's rocking-horse. Frisch had been aware of Slutsky's work earlier than most others. Frisch and Slutsky had been in correspondence and exchanged reprints for a couple of years before 1927[14]. Slutsky had sent a reprint of his 1927 paper to Frisch in New York in May 1927[15]. Frisch could not read Russian, but the English summary was sufficient for him to immediately see the importance of Slutsky's results[16]. As a passionate numerical analyst himself he could appreciate the vast effort Slutsky had put into the paper[17]. At the meeting Frisch elaborated only very briefly on these concepts, perhaps finding it too difficult for the audience. He explained the impulse-propagation as kicks given to a pendulum. A real strong impulse (kick) could force the pendulum and cause a perturbation and thus a *half-free oscillation*.

Frisch had thus clearly worked a lot the implications of Slutsky's while he was in the United States. Slutsky's numerical experiment raised doubt that "observed" cyclical fluctuations were entirely spurious, created by mechanical smoothing or more generally any kind of linear operators applied to a random series. This was a valid point, hence a need for time series analysts to safeguard the procedures from this pitfall. But it was another aspect that could be derived form Slutsky's result that Frisch was more concerned with, namely that the economic structure, conceived as a linear dynamic model, worked as a linear operator on random disturbances and cumulated their effect as cycles. Frisch was determined to penetrate this problem such that he could find the exact nature of the cycles generated when a given linear operator was applied to random shocks. How far he had advanced along this line by 1931 is unknown, but he would make it a major issue in the ensuing years.

Then Frisch moved on to modelling issues. Without equality between the number of conditions (equations) and the number of variable to be explained by the model, no theory could adequately explain how the successive phases of the cycle followed from each other and thus would have to refrain from completeness in the explanation. This was, indeed, the case, the typical cycle explanation would, according to Frisch, assume that something, say the price level, moved cyclically, and from that try to explain the cyclical movements in other variables. Without referring to any specific business cycle theory Frisch asserted (point 7) that they all across the board were indeterminate. The way of getting around the determination problem in the explanation of, say, the turning points of the cycle, was always the same. At the decisive point in the analysis "… one

14 Frisch was one of few people who had received from Slutsky his famous 1915 paper, not yet rediscovered!

15 Slutsky intimated to Frisch what the paper had meant to him in human terms: "… ich während aller dieser Zeit so mit einer Arbeit eingenommen wurde, so in der Arbeit lebte, dass ich für Alles in der Welt fast vollständig blind und stumpf wirde. Krank war ich - fast könnte man sagen - mit dieser Arbeit. Meine Freunde Scherzten., dass ein Wurm "Gordius stochasticus" (siehe Fig. 10 und Fig. 12) mir an der Mark frass und mein Blut saugte." (Slutsky/Frisch, 9 May 1927).

16 "I am sorry I am not able to read the 'Summation of …' in extenso. Anyhow the summary is sufficient to show the extreme importance of your problem. I have found your treatment very suggestive indeed. It can be no doubt about the fact that you have here a very fruitful idea the following up of which seems highly promising." (Frisch/Slutsky, undated answer to Slutsky/Frisch 9 May 1927).

17 Soon after Frisch sent Slutsky a copy of his own paper (Frisch, 1927). Slutsky also drew Frisch's attention to Yule's paper published the same year (Yule, 1927) and even sent Frisch his recent correspondence with Yule. Frisch's appreciation of Slutsky (1927) is also apparent from the fact that practically his first act as Editor of Econometrica was to ask Slutsky's permission to publish the paper in English. He addressed Slutsky 10 months before the first issue of Econometrica appeared, but five years would still pass before the article was finally revised, translated and published (Slutzky, 1937).

stretches the hand out and and pulls in a new variable by which one 'explain' that the turning point in the others will occur"[18]!

Then Frisch offered a prescription (point 9) for setting up a proper business cycle model, reiterating some of the points above. The dynamic character of the proposed model, Frisch's *shape problem*, cannot be ensured ex ante, but requires calibration (estimation) of the parameters. Frisch also included an example in the handout of a dynamization of simple demand-supply scheme to "illustrate the nature of the theoretical tools necessary for a properly determined business cycle theory". Finally, Frisch offered (point 10) nothing less than a sketch of a large business cycle model (see below).

In the "statistical part" Frisch first (point 11) referred just briefly to the decomposition problem, i.e. the topic of Frisch (1927, 1928). At this time he had revised his view of some of the original methods and suggested new directions in Frisch (1931).

His next issue was a highly interesting one, as it touched upon the theme of his 1938 paper. The terminology was a little different. By '*prim-relation*s' Frisch meant autonomous relationships: "When we try to verify economic-theoretic laws by means of statistical data, we are in the strange situation that if our data satisfy several prim-relations, then these data can in principle <u>not</u> be used in the determination of these prim-relations." The (deflated) *confluent* relations or *phase-relations* (in 1938 called *coflux* relations) on the other hand, can always be determined. This was the problem of passive observations simultaneous fulfilling several relationships. He must have spoken over the heads of the audience on this topic, although he tried to exemplify by some very simple mathematical examples.

He reiterated a favourite criticism he had raised before, that many parameters, say of demand elaticities, which were "estimated" in the literature, were in fact estimated as the ratios of two error terms and thus fundamentally indeterminate[19]. But there were ways out of this dilemma, namely "in situations where one can introduce an additional variable about which it is plausible to make distribution assumptions". Another way was a pet idea of Frisch – the interview method.

What then was Frisch's model? It was specified as list of 38 (endogenous) variables followed by a list of 37 equations, defined only suggestive verbal formulations. Hence, it can hardly be called a properly specified model. Frisch confessed without embarrassment that he had not been able to find the missing equation! He had left out a number of *accessoric variables*, such as various aggregates, that could be added as variables and definitional equations at the same time, and indicated that some of the variables and equations were too aggregate. Hence, it was a really large model sketch he had outlined. Several key equations were just suggested by names such as "the supply function for labour", the demand function for consumption gods", "the general price level", etc. The lack of corroboration was very un-Frischlike and suggests that the whole model sketch had also been drawn up at short notice. It is thus difficult to work out if the model made sense[20].

In the ensuing years the business cycle project was a major priority at Frisch's University Institute of Economics, established from January 1932, financed by Rockefeller foundation. Needless to say, the business cycle project was a major priority. When he reported to Rockefeller Foundation after four years, half of the money and time spent was on the business cycle project[21]. The report dealt with the project under the following five sub-headings, indicating how Frisch structured the overall project.

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18 Tinbergen's long survey of business cycle theory in Econometrica (Tinbergen, 1935) confirmed Frisch's view on this point. He found only two mathematically formulated closed system in the literature, Frisch's and Kalecki's theories, both presented at the Leyden meeting of the Econometric Society in 1933, and subsequently published.

19 He would elaborate much more on this topic in his Pitfalls criticism of Leontief and in his Confluence Analysis.

20 The model sketch is mentioned in Andvig (1986), who dismissed it as "Gargantuan" (p.80).

21 Frisch (1936). The other half was divided between "Productivity Studies", "Demand and Utility Studies" and "Statistical Technical Studies". Each of the two first headings covered a number of empirical studies, the last one comprised the confluence analysis.

1. The mechanical decomposition problem.
2. The creation of cycles by the cumulation of erratic shocks.
3. Why an economic structure acts as a linear cumulator. The role of economic theory in business cycle analysis.
4. The structural decomposition problems.
5. The problem of structural forecasting

The first sub-heading was the old decomposition problem. He concluded tentatively about this part of the project that it was more difficult than originally conceived, adding that is "has never ceased to occupy my mind". Once the simplified assumptions underlying the periodogram analysis were abandoned "the problem becomes of tremendous complexity." The original ideas in Frisch (1927) had not worked out as well as expected and needed "a more careful formulation". Frisch explained to Rockefeller Foundation that the most important advances towards an understanding of the conditions under which the mechanical decomposition problem had solution had been done by the study of constructed data. But he could not show much of a gain from the work. His concluding statement sounded more like a confession of belief: "I am more than ever aware of the great difficulties of the problem and also of the many pitfalls that exist in this field, I am confident that progress is possible."

The second sub-heading was the problem inspired by Slutsky. An enormous amount of numerical simulation had gone into this project. His best students, Trygve Haavelmo and Olav Reiersøl, had worked on it. Part of the Norwegian documentation of the project was in the form of (incomplete) lecture notes[22]. In the report Frisch expressed his conviction that observed cycles were in fact created by the Slutsky effect. He asserted that the work at the Institute had succeeded in constructing experimental models of series which resembled very closely those actually observed. Frisch had been determined to penetrate this problem such that he could find the exact nature of the cycles generated when a given linear operator was applied to random shocks. In the report he claimed that he was "in possession of a theory which answers in a fairly complete way most of the questions that are of interest in this connection", adding that most of the manuscript was ready and could be brought out at short notice. This was undoubtedly the publication he had promised on so many occasions, but in 1936 he still preferred to wait to publish until he had results also from the other parts of his project.

When coming to the role of economic theory in businesss cycle analysis, Frisch's use of the Slutsky effect implied that the time shape of the evolution of the economic variable was not determined by the economic structure per se, but the economic structure defined instead the weight system by which the random shocks were transmitted. An economic model with dampened oscillations would exposed to random shocks, have the oscillations maintained with irregularities. Thus, typically, one would find cyclical components corresponding to the dampened cycles of the model, i.e. the rocking horse, but besides this also other components could be present, caused by the Slutsky effect. The amplitudes and other proeprties would be explained by the theory that Frisch had developed.

Frisch seemed fascinated by the dual nature of the cause of the cycle, one part explained by economic theory, the other part by statistical theory: "Personally I am convinced that it is only through a combination of these

[22] Frisch (1934b). The section headings of these lecture notes may have some interest: (1) Preparatory part: generally about time series and components, (2) Linear operations, (3) Quadratic operations, (4) Factorization algebra, test determinants and key equations, (5) Cycle creation by erratic cumulation, (6) Linear operations and key functions adaptation according to the nature of the data, (7) Decomposition in practice, examples, (8) Decomposition techniques as a means for smoothing.

theories together with an intensive utilization of actual data that a realistic explanation of economic phenomena connected with the business cycles can be arrived at."

He outlined a research strategy where he would test out various types of theoretical schemes and then determine by means of this theory what kind of cycles the scheme would lead to ("… a systematic scrutiny of this sort is at the present time one of the most urgent needs in business cycle analysis"). He mentioned the Cassel essay from three years earlier, but almost apologetically as "a small study which may be mentioned in this connection".

Having conceived the generation of business cycles as set out above, he posed as the structural decomposition problem to invert this process. Can from a given series, produced through the Slutsky effect by a given model, the shape of the weight curve by which the random occurrences have been accumulated, be determined? And further, can the individual random disturbances be determined as well. Frisch's line of attack was theoretical analysis and numerical simulations. He had developed a number of methods which he subsequently tested out, but admitted in his report to Rockefeller Foundation that "it goes without saying that a number of ideas thus suggested has turned out to be valueless." He added on a more hopeful note that "through these assiduous tests certain general lines of approach now begin to stand out as fundamentally sound." But did this really mean that he saw light at the end of the tunnel, or was he just defending a wasted effort?

The inversion problem was however, very important in his overall strategy, because the ultimate purpose of the business cycle project was after all forecasting. Frisch's conception of forecasting relied completely on the structural decomposition. The forecasting itself was simple, namely, take the weight system as determined in the decomposition, include the effects of random shocks in the past and project the further development of the system on the assumption that no more random disturbances occur. The uncertainty of the forecast could also be assessed through the clear separation in this problem of what was known and what was not known. Forecasts further into the future would be increasingly indeterminate. In his report Frisch referred to "attempts at forecasting by this method have been made at the institute with a fair degree of success", but clearly this part of the work could not have had advanced far as long as the inversion problem was not properly solved.

At about this time the steam must have gone out of the project. From 1936-37 the Institute got involved in a government sponsored project to investigate the problems and prospects of the Norwegian economy. Frisch took upon himself to be in charge of developing Norwegian national accounts, an idea he had proposed some years earlier.

In the aftermath of these intensive four years of work on the business cycle projects some further incidents may be mentioned. In September 1936 Frisch took Haavelmo with him to the Econometric Society meeting in Oxford, at which the discussion of Keynes' *General Theory* was the big topic. Frisch seemed to have kept a low profile (unusual for him) in this discussion. His own paper was titled "Macrodynamic Systems leading to Permanent Unemployment", drawing on a model of a corn economy he had lectured on, but remote both from the Keynesian model and from his business cycle project. Frisch's concluding remark, according to the report, was that as there already existed 10-15 fully-developed mathematical systems of some plausibility "the task was no so much to develop new systems as to test different systems against the facts". Jan Tinbergen who was already well ahead in his project for the League of Nations spoke on "Dynamic Equations Underlying Modern Trade Cycle Theories". Later during the meeting Frisch unannounced presented an "ideal programme" for macrodynamic studies which reflected the conclusions he had reached in the business cycle project[23].

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[23] Frisch's "ideal programme" is discussed in Aldrich (1989).

In the autumn of 1937 Tinbergen visited the Institute for about one month. Although no documentation is available about the discussions during the visit, one has to presume that Tinbergen's League of Nations project was the main topic. Shortly afterwards Haavelmo left the Institute for studies abroad, visiting Berlin, Geneva and Paris. The Berlin visit was an assignment from Frisch as Haavelmo brought constructed data with him to test the more advanced equipment for time series available in Berlin at the Institut für Konjunkturforschung, directed by Ernst Wagemann, and the Meteorologisches Institut's department for 'Periodenforschung', directed by Professor Karl Stumpff which had a range of advanced equipment for harmonic analysis. Haavelmo tried Stumpff's equipment on data that had been analysed in Oslo by Frisch's methods to compare the efficiency[24]. Stumpff's harmonic analysers were based on light interference and were described in a note by Haavelmo[25]. Haavelmo was not impressed with the results, they were hardly as accurate as the results achieved in Oslo. On the eve of his departure from Berlin he sent home his report concluding that the methods were useful as they required little work, even with several components included in the series, but they were not able to solve Frisch's inversion problem. Frisch studied the results and concurred.

Frisch was invited to the Cambridge meeting convened 18-20 July in 1938 to discuss the results of Tinbergen's project and sent his note Frisch (1938) too late to arrive in time for the meeting. The note reached Tinbergen who wrote a reply which raises some doubts as to whether he had understood Frisch's criticism correctly. Frisch's note was a bit strange as he did not really comment upopn any equation or result by Tinbergen, but asserted very generally that Tinbergen's work could not be considered as test of business cycle theories. Tinbergen had estimated relations, but Frisch clearly doubted the connection between the theoretic rationale Tinbergen had given his equations and the estimates he had produced. In Frisch's view Tinbergen had not been able to estimate any autonomous relation, due to the confluence caused by simultaneity, while Tinbergen did not seem neither to understand nor accept Frisch's assertions.

Jan Tinbergen, who of course also was a strong "methods man", differed perhaps from Frisch also in his attitude towards the subject matter. Mary Morgan gives an apt expression to this difference when she writes: "Good intellectual reasons apart, Frisch, unlike Tinbergen, preferred to work on problems of the methods and methodology of econometrics rather than on applied econometrics using real data[26]."

If nothing else, the war killed Frisch's business cycle project. After one year's imprisonment during the war Frisch was in very good shape at the liberation, ready to devote his entire efforts to continued research. The new keyword in Frisch's work was decision models. In a classification of modelling ambitions he introduced in a somewhat derogatory sense the term "onlooker models" for any modelling effort that was not meant for policy analysis. Presumably, it would cover also his own previous efforts.

4. The disappearance of the cyclical movements

What happened to the business cycle research after the war? This sort of economic analysis continued to be the responsibility of the Central Bureau of Statistics. Moreover, most branches of economic research were strengthened considerably in the early 1950s. In 1949, the Frisch-student Petter Jakob Bjerve, was appointed General Director, and he kept this position until he retired in 1980. Bjerve soon established a separate research department within the Bureau, which came to be occupied with national accounting, research on taxation issues, construction of input-output tables and macroeconomic models for policy-making, and business cycle studies (Bjerkholt 2000a, 2000b).

24 Frisch had constructed test data sets from drawings of a Norwegian lottery ("Pengelotteriet") and sent data to Haavelmo by mail. The data were held up for a while by the German censorship suspecting that the data were ciphered messages!

25 Stumpff's methods had been set out in his book Grundlagen und Methoden der Periodenforschung, to which Haavelmo made page references.

26 Morgan (1990, p.98).

The Bureau kept on publishing a number of statistical series on a monthly basis, labelled the "konjunkturtabell". This table was expanded during the 1950s and new short-term indicators for specific parts of the economic life were produced. During the 1950s and 1960s, the Bureau took part in the OECD cooperation around the development of new methods for removing seasonal trends and later for developing leading indicators. In the three decades from 1945, the short-term statistics was expanded and methods refined. Still, the big change what we might call business cycle analysis came in the interpretations and presentations of the short-term statistics.

In very general terms, the main project for the Research Department in its early years was to establish a coherent system for macroeconomic policy and analyses. The meeting point for the establishment of statistical standards, of new economic concepts, and practically oriented discussions of aims and priorities, and failures and successes, in economic policy, was the national accounting system. In Norway, national accounts were produced on a quarterly basis already from 1953, as a part of the short-term analyses of the economy. (A large part of the abovementioned expansion in short term series and indicators were made in relation to the quarterly national accounts.) Thus, the business cycle analyses became closely integrated with the national accounting system, and, although more indirectly, with policy advising and policy analyses.

In some respects, Eilif Gjermoe fate illustrates the role of the inter-war analysis of economic cycles. From the start, Gjermoe had little contact with the new leadership in the Bureau and the newly established Research department. He resigned as head of his department in 1952 but he kept on making retrospective studies of the business cycles (Gjermoe 1951, 1953, 1955). However, the Bureau did not publish these writings (as they have used to do with the writings of distinguished retired researchers). But an other study of the same period was made in the Bureau by a young economist (later professor in economics) Gerhard Stoltz, in the early 1950s, *Economic Survey 1900-1950* (Stoltz 1955). The book was printed in the Bureau's new series for important research publications, and Stoltz must have written most of it in Gjermoes last years in the Bureau. Still, Gjermoes writings were not mentioned or referred to at all in this book. Neither did Gjermoes basic unit of analysis, the cycle, play any role in the analysis. Like other retrospective studies of economic development in the Norwegian economy, the long-term trend was the focus, and exceptions, called "crisis" or "setbacks", were commented in words and figures[27].

The major study "The Norwegian Post-War Economy" published in 1965 had a short chapter with an overview of the "business cycles" *(konjunkturbildet)*[28]. The Bureau explained that the situation at that point of time was entirely different from the 1920s and 1930s. No deep crisis like the one in the 1930s had been experienced, nor had there been any cyclical movements like those seen in the inter-war years. The shared reason for these changes were explained like this: "It is in the economic policy one must search for the main cause of this improved economic development […] The breakthrough in the field of theoretical economics in the 1930s, (exemplified with Keynes), national accounting, econometrics, and making of quantitative economic models have been important steps in this development" (CBS 1965: 341 ff.). In a wider perspective, this vision of past and present must be seen in relation to the long governing Labour Party's rhetoric in the filed of economic management: Crisis and cycles could be avoided by demand management and regulation of various kind, and for example the impact of the depression in the 1930s was described as a consequence of hamstrung and incompetent liberal and conservative government. – Also Frisch's turn from business cycle analysis to his attempt to construct decision models can be seen in the light of these developments. Frisch's left his cycles

[27] In addition to Stoltz (1954) but also CBS 1965 and CBS 1966 ought to be mentioned.

[28] Norway and Scandinavia use the German term konjunkturer for 'business cycles'. In the inter-war years one talked about 'konjunkturbølger', in German-English 'konjunktur-waves'. What happened after the war was that 'waves' (bølger) disappeared in the languages used, as well as in the analyses.

– the inter-war unit of analysis – and concentrated on economic models open to demand management and other types of economic governance.

In other words, in the 1950s and 1960s the Bureau and other hosts of modern economic theory and techniques had eliminated the cycles – not only as a unit for analysis, but as a reality of economic life. Today, many expert commentators would prefer a more modest variant; that the "cycles" became less evident because of changes in technological potentials and transfers, and in trade patterns. Yet, it is interesting to note how Stoltz and later several high-ranking economists in the Bureau also disregarded "cycles" as an analytical term when they investigated the long-term development of the Norwegian economy, even in the periods before the macroeconomic enlightenment had waved the waves farewell. Moreover, the "cycles" re-emerged in the 1970s. In the more turbulent years from the early seventies, the post-war optimism on behalf of economic management was gradually weakened. In 1978, Kjell Wettergreen, the person responsible for konjunktur-analysis in the Bureau, published a study titled "Konjunkturbølger i norsk økonomi" (International cycles in Norwegian economy[29]). The study covered period from 1955 to 1976. In this book, the cycles ('bølger') existed, not only in the seventies, but even all the way back to the mid 1950s. In the foreword, Wettergreen explained to his readers that he had tried to identify "turning points", as this was the recommendation given by the international expertise in this field.

At the time Wettergreen made his study, the now rather unknown Gjermoe was still active[30]. From time to time he published short statistical surveys in a working paper series of the Institute of maritime economics at the Norwegian School of Economics and Business Administration in Bergen. His field of study since the late 1950s had been the development in the shipping sector. In this sector, national oriented macroeconomic data and analysis had little relevance. And, of course, shipping industry was the sector where even Norwegians still talked about "cyclical movements" in prices and volumes.

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29 Wettergreen himself was not a new-comer in this field of analyses, but had worked in the Bureau in this area since 1960.

30 Not only for Norwegian economists has Gjermoes writings been regarded as irrelevant. In the late seventies and early eighties, a larger economic history project was launched to investigate the development of the Norwegian economy - the manufacturing industry in particular - in the 1930s. The questions asked were strikingly similar to those Gjermoe dealt with in his studies. The historians and social scientist involved were obviously unaware of Gjermoes research. Anyhow, after some years they ended up with a set of conclusions nearly exactly similar to Gjermoes (1935,1936,1938, 1951) with regard to the how the sectors were affected

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THE SHORT-TERM ECONOMIC ANALYST, THE NATIONAL ACCOUNTANT, THE ECONOMETRICIAN AND THE PLANNER: CONTROVERSIES ABOUT FORECASTING IN FRANCE AND THE NETHERLANDS (1930-1980)

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1. Abstract

Since the 1920s, the efforts to describe and forecast economic fluctuations on the basis of statistical series have led several countries either to broaden the functions of government statistical offices or to set up new agencies dedicated to short-term economic analysis (*instituts de conjoncture*). The methods used for these purposes have evolved, sparking many debates. Our paper examines the birth and dissemination of three such methods in two countries: France and the Netherlands. The three are not, of course, mutually exclusive, but they have often been wielded against one another in different ways in each country.

- The first method, that of the short-term economic analysts, was exemplified in France by Alfred Sauvy and Jacques Méraud. It consists in examining intra-year series – typically monthly – supplemented since the 1950s by confidence surveys among business executives. The consistency of the approach is "qualitative" and is backed neither by book-keeping balances nor by econometric modeling.

- The second method, that of the "national accountants," is symbolized in France by Claude Gruson. It involves the compilation of annual national accounts, which are then "projected" for the current or following year into "economic budgets" (*budgets économiques*) or in medium-term forecasts (planning *à la française*). These consist of annual-flow forecasts (often based on expert opinion) expressed within the national-accounts framework, which is assumed to be consistent and comprehensive.

- The third method, associated with the Dutch economist Jan Tinbergen, relies on an econometric modelling of regular patterns observed in the past. It appears in the Netherlands in 1936, and is used in dynamic planning, very different from the French planning used until the end of the 1960's.

The three methods were developed in very different ways in France and the Netherlands between the 1930s and the 1970s. Both countries have witnessed controversies between the advocates of each. This paper seeks to relate the debates to their national contexts. We try to explain how and why the methods for studying current economic trends and preparing economic forecasts diverged so sharply between the two countries.

2. Difficulties and pitfalls of a historical comparison between two countries

In 1946, two European countries, France and the Netherlands, emerged from the same tragic ordeals and began to reconstruct. For this purpose, each established an "economic planning" agency: the Commissariat Général du Plan (CGP) in Paris, and the Central Planning Bureau (CPB) in The Hague. The political context, the debates prior to their establishment, and the goals assigned to these innovative institutions all resembled each other. Both agencies were supposed to implement an "indicative," "consensus-based" planning focused on decentralized forecasting and decision-coordination. Their common goal was to rebuild a free market after the gradual dismantling of wartime bureaucratic regulations. Both explicitly distanced themselves not only from the authoritarian command economies of the Soviet Union and Nazi Germany, but also from the refusal (as a matter of principle) to even use the word "plan" to denote a public institution. Such reluctance was observed in the postwar U.S., Britain, and Germany, even though all three countries did contemplate some form of economic-policy planning.

However, in spite of their comparable initial choices – which set them apart from their counterparts in other countries – the Dutch and French planning systems diverged on a key point. In the Netherlands, econometric models of the overall functioning of the economy were developed by the early 1950s. Their results have been widely quoted and discussed in the major social and political debates, elections, and varied crises that have marked the country's history in the decades since the war. In France, by contrast, such models have been built and used – nominally along the same lines as in the Netherlands – only since 1970 or so, and their role in the social debate has never been as extensive. In fact, it has sharply diminished since the early 1980s. Can such a difference be explained? This question may provide an opportunity for an exercise in comparative sociology, to which we shall apply two types of instruments: the first pertain to the internal history of technical tools for economic description and analysis (conceptual frameworks [economic and non-economic], statistical methods, national accounting, and econometrics); the second are drawn from the external, social, intellectual, and political history of the two countries[1].

As it happens, the very first macroeconometric model was built – back in 1936 – by a Dutchman, Jan Tinbergen, specifically to address economic-policy issues relating to unemployment and the foreign-trade crisis of the 1930s. Moreover, Tinbergen was the CPB's creator and first director from 1946 to 1955. This circumstance makes a comparison between France and the Netherlands even more interesting, but also raises a tough question for the analyst who wants to adopt a historical sociology perspective: can the differences be explained by the personality and role of a single individual, however outstanding? This exercise on a closely defined issue may therefore lead to a wider examination of the links between a (micro)sociology of science and technology, as practiced today with increasing frequency, and a (macro)sociology, more classical and historical, focusing on national institutions, cultures, and groupings. This paper seeks not only to answer a specifically historical question on France and the Netherlands, but also to examine a problem of sociological method: what elements are deemed relevant in addressing such a question? Indeed, what does it mean to "explain" such differences? Can one do anything other than select items and organize them into a narrative that will take the shape of a network of facts held together more or less successfully?

But one can build very different narrative networks. Indeed, the explicit elaboration of these multiple narratives is a crucial step in comparative analysis. We can see this, for example, in the diversity of documentary and historiographic sources already available. There is a wealth of literature on the history of statistics (INSEE 1987) and national accounting (Fourquet 1980) in France. For the Netherlands, by contrast, there has been less ground-breaking research on these topics (de Vries et *alii* 1993), but the literature on the origins of Dutch econometric models is abundant given their role (and that of Tinbergen) in the general history of modeling (Bodkin, Klein, and Marwah 1991; van den Bogaard 1998). Studies on the French models exist as well, but are of course more modest. This historiographic difference is both significant (it anticipates the findings reported below: an emphasis on national accounting in France, on modeling in the Netherlands) and a hindrance to a term-by-term comparison.

The diversity of possible narratives is part of the problem that needs to be addressed. While keeping this factor in mind, we will present a selection (among several) of micro – and macro – social features that provide the outlines of two scientific, administrative, and political constellations. The Dutch breakthrough in modeling owes much to the complex, multi-faceted personality of Jan Tinbergen. For this reason, we will center the analysis of the French situation on a few comparable individuals, but each will be examined from a different angle: Marcel Lenoir, E. Dessirier, Alfred Sauvy, Jean Monnet, Claude Gruson, Jacques Méraud and Edmond Malinvaud; we will also mention the part played by other Dutch figures.

[1] The following analysis owes much to information provided by Dutch researchers: Marcel Boumans, Johan Heilbron, Frank Kalshoven, C.A. Oomens, and Adrienne van den Bogaard. Special mention must be made of Jan Tinbergen's biographer Albert Jolink, who taught me much about him. Jolink allowed me to accompany him on one of his regular visits to Tinbergen. Shortly before his death in 1994 at age 91, Tinbergen was kind enough to answer my questions, in the perfect French once spoken by European intellectuals. In France, Edmond Malinvaud, Jacques Mayer, and Pascal Mazodier also replied to some of my queries. My thanks go to all mentioned.

The two narratives are fairly independent to the extent that, despite similar goals and planning, there have been few exchanges between the two countries. Reciprocal references are rare, and the protagonists, in both the academic and political spheres, have little knowledge of one another. French economic forecasting has long been inward-looking. The Netherlands has been more open, first toward Germany, then toward the English-speaking world, but scarcely toward France. Behind this lies a major factor: France sees itself (rightly or wrongly) as an intellectually sufficient "biggish country" — in contrast to the image of the Netherlands as a "small country" with a strong trading tradition and a more spontaneous openness. The varying degree of closure of the technical-political networks represented by the two countries seems to justify macro-social analyses of features of the two national cultures that we can assume to be deep-rooted and long-term. It may therefore be tempting to summarize them in simple expressions that are easy to memorize and transmit, potentially for purposes of argument or condemnation, for example on the "backwardness" or "archaism" of France (from the standpoint of econometric modeling). To multiply the points of view shows that such summaries are intellectually stimulating, but also risky. More prosaically, the diversity of narratives and the selection of the features presented also depend, in a partly random manner, on the information sources available: one cannot neglect this contingent nature of historical analysis. But our task, after all, is to provide food for thought more than to reveal a hitherto hidden truth.

3. A combination of eight different approaches

The macroeconomic modeling taught today and used all over the world rests on a common core of methods and constraints, even though the economic hypotheses and statistical techniques differ from one model to another. To understand how and why two apparently similar countries elaborated such models at very different times and following very different paths, we need to remember one important fact: this common core – now well-formed and accepted – is the historical product of the combination of many cognitive and institutional tools, which are relatively independent of one another. This combination was achieved gradually, and there was no prior script showing the order and the form in which the different approaches were to link up with one another in succession. In the 1950s, the Klein-Goldberger model was the temporarily unified synthesis of these approaches, and it later spawned a wide variety of other models (Artus, Deleau, and Malgrange 1986).

We can break down this now solid core of economic modeling into eight approaches issued from highly different paths[2]. Without charting this complicated genealogy in detail[3], we must stress that, depending on the circumstances, a particular approach or a particular combination of some of the approaches has been highlighted in order to characterize the essence of a modeling method, or the defining moment (and the toughest watershed) in a historical sequence. That is precisely why the history can differ from one country to another, the term "history" being used with its two possible connotations: as reality (history as it "really" happened), or narrative (history as constructed and recounted). As an academic exercise, one could show that the history of modeling can be told in eight different ways, by successively following each of the strands originating in the eight ideas. These are listed here merely to show how they advance and combine differently according to the national context.

2 There is nothing absolute about the figure eight. Other approaches could certainly be added to the list.

3 Much information on it will be found in Stigler (1986); Morgan (1990); Bodkin, Klein, and Marwah (1991); Armatte (1995); and Desrosières (1998).

3.1 Quantification and observed regularities

Economic and social phenomena can be quantified and described by "statistics," and these measurements are endowed with regularities and constancies over time. That is the central idea propounded by Quetelet and fought by "statisticians." The latter either denied the very possibility of equivalences and additions (the eighteenth-century "German" approach to statistics) or accepted quantification but denied the existence of stable, regular patterns (Moreau de Jonnès, founder of the Statistique Générale de la France [SGF] in 1833). Meanwhile, the theoretical economists (Say, Walras) were rather suspicious of the gap between abstract concepts and concrete statistical measures whose purity cannot be guaranteed (Ménard 1987).

3.2 Markets and their general equilibrium

The economy can be conceptualized as a comprehensive system of market transactions, whose participants interact in conformity with general laws expressible in mathematical form. This is the general equilibrium described by Walras and Pareto. The notion does not imply *a priori* that interactions and their regularities can be effectively measured. It is a formal construct whose very strength is based on this possibility of turning the general market laws into abstractions. By the late nineteenth century, reliance on statistics was perceived as creating the risk that economic thought would backslide into contingency and historicity. A controversy erupted between the German historicists (and their U.S. institutionalist descendants down to Wesley Mitchell and Rutledge Vining), who were heavy users of statistics, and the "hypothetico-deductive" and mathematical economists. The quarrel specifically centered on the opposition between descriptive statistics and mathematical formalism, which were regarded at that time (late nineteenth century) as incompatible (Armatte and Desrosières 2000).

3.3 Short-term economic analysis

Temporal regularities do not necessarily imply the constancy postulated by Adolphe Quetelet. Historically observed fluctuations can be analyzed as such, and connections can be drawn between them. They are often *cyclical*. The cycles can be described and in some cases "explained" by external "causes" (Clément Juglar, Henry Moore). This notion led to the construction of "economic barometers" (Harvard) and the founding of organizations dedicated to the analysis of current economic trends (*instituts de conjoncture*), which flourished in the 1920s and 1930s. In France, Dessirier and Sauvy attempted-with scant success-to promote the language of "short-term forecasting" (*prévision conjoncturelle*). These circumstances fostered the development of graphic-representation methods, moving averages, and seasonal adjustments. A shade of difference appeared among these "short-term economic analysts" (*conjoncturistes*) between those who discerned, in each phase or cycle, singular moments warranting specific analysis (Mitchell), and those who sought to register empirical regularities in stable forms, thereby anticipating econometrics (Lenoir 1913; Moore 1914).

3.4 Mathematical statistics derived from biometrics

Indeed, regularities and statistical relationships could themselves be formalized and "measured" with the tools of incipient *mathematical statistics* (regression, correlation, tests), derived from biometrics (Francis Galton, Karl Pearson). These tools were imported into the social sciences and economics, first by Yule and Bowley, and later in the form of probabilistic inferential statistics (estimations, tests) by Fisher and Neyman-Pearson. In the early days of econometrics (which did not yet bear that name), between 1900 and 1930, these first non-probabilistic tools were imported for the purpose of estimating the laws of supply and demand and analyzing cycles (Lenoir in France, Benini in Italy, Henry Moore and the Working brothers in the U.S.).

3.5 National accounting

Economic life can be summarily measured and described through broad "aggregates" such as national income, gross national product, and consumption. This notion of *national accounting* successively developed in three different and partly unrelated ways, each linked to one of the three approaches still used today to determine GNP: (1) The measurement of *national income* forms part of an analysis of income: wages, profits, and rents (Bowley, in the early twentieth century). (2) The measurement of a national product proceeds from an analysis of output by economic sector (Wesley Mitchell, Simon Kuznets, and Colin Clark in the 1920s and 1930s). (3) The measurement of *demand* components (consumption, investment, public spending) is tied to the dissemination of Keynes's ideas on the macroeconomic equilibrium between overall supply and demand (1940s). The theoretical conceptualization of national accounting, and the cumbersome, expensive establishment of the statistical infrastructure needed for its effective implementation, have been viewed in diametrically opposite ways. In France until the 1970s, they were seen as the defining moments in macroeconomic modeling. In the Anglo-Saxon countries, on the contrary, they were almost entirely downplayed as a purely technical step without historical significance or major scientific implications.

3.6 Economic dynamics

The time variations in the components of the economy – interlinked by the predefined book-keeping balances of the national accounts – may be described and analyzed with the tools of descriptive statistics listed in 3.3 above. But they may also be viewed in terms of *dynamic sequences*. That was the main contribution of Tinbergen in the 1930s. His exercise combined several different conceptual approaches: overall systemic equilibrium, itself a synthesis between a Walrasian theoretical economic equilibrium (3.1) and an empirical, book-keeping equilibrium (3.5); time analyses (3.3); and mathematical statistics applied to economics (3.4) – not to mention even other formalisms, derived from physics and mechanics (harmonic oscillators, differential equations). This dynamic aspect was not fully integrated into French modeling until the mid-1970s (DMS and METRIC models).

3.7 Models become probabilistic

Econometric relationships, estimated either statically or dynamically, can be incorporated into a *probabilistic model*, which allows two things: (1) a conceptualization of the relationships between theoretical economic hypotheses, observed empirical regularities, and the heterogeneity of isolated cases; (2) a solution to the question of the simultaneity of the estimated relationships. This "probabilization" of models was absent from Tinbergen's work in the 1930s. It was fully elaborated by Trygve Haavelmo (1944) and the Cowles Commission in the U.S. (Morgan 1990), one of whose key members was the Dutch economist Koopmans. At that point, a synthesis was achieved with yet other conceptual approaches based on probability theory as a branch of mathematics, via the Britons William S.Gosset (Student), Ronald Fisher, and Egon Pearson, the Pole Jerzy Neyman, and others. Although some French specialists (Emile Borel, Maurice Fréchet, Georges Darmois, and Paul Lévy) played a central role in developing probability calculus, they do not seem to have participated significantly in its introduction into economic models. In France, this formalization was undertaken by Edmond Malinvaud in the 1950s.

3.8. A benchmark for economic policies

The notion of closely linking theoretical speculations (economic or mathematical) to series of statistical and accounting data did not gain acceptance until the 1930s and 1940s. It was also then that such constructs were explicitly presented as necessary for guiding macroeconomic policies in the dramatic context of the Great Depression, the war-related mobilization of industry, and the post-war reconstruction. This almost militant rationale was strongly defended by the creators of the first models. It is comparable to the physicists' engagement in the Manhattan Project to build the atom bomb, in terms of the costly mobilization of vast teams as well as the swift progress in scientific research that resulted. This direct involvement in public affairs – henceforth conceptualized through the new tools – led to a change not only in the scale but also in the nature of the now tight-knit combination of the seven approaches described above. Depending on the country, however, the links were drawn and articulated in different ways. As we shall see, it is not enough for a new idea to be formulated at a given time in a given country. It will not produce effects later on unless it is capitalized and embedded into stable networks, both cognitive and political. In what conditions can this be achieved? The decisive factors are the relative weights of, and interactions between, universities, research centers, statistical offices, other government agencies, political institutions, and even corporations, labor unions, political parties, and churches. This configuration differs from country to country.

4. Governments and universities: a specifically French division

The content, style, and importance of economic forecasting in a country's administrative and scientific space give a good idea of the implicit role of the State, particularly as regards scientific and technical expertise. In some countries, the expertise lies more or less outside the government establishment; it is found in universities or scientific centers, which, even if they receive public funds, maintain contractual relationships with government-the two sectors pursuing distinct goals. France displays a more complex profile. To begin with, socially recognized technical expertise – and even scientific expertise – has long been (and to some extent still is) internal to the State. Its "carriers" are the cadres of engineers (*corps d'ingénieurs*) graduated from the élite *grandes écoles*, whose prototype is the École Polytechnique. As a result, the university has played a lesser role, as is clear in the teaching of mathematical economics (Le Van-Lemesle 1991).

The French tradition of science in the service of the State has long had decisive consequences[4]. The engineers' expertise, geared to administrative action, tended to fragment and specialize in their distinct fields of involvement. Circulation between fields was unlikely. For example, by the late nineteenth century, French artillery officers had developed high-level probabilistic and statistical formalisms for their own use (Crépel 1994) – but these were unknown to contemporary statisticians and economists. Another example: the exchange of ideas between physicists and economists – which triggered the work of Tinbergen and Koopmans – would have been hard to imagine in France. The administrative focus was not an incentive to the accumulation of knowledge in a systematized form. Bibliographies were slim and haphazard. Exchanges with other countries were infrequent, or confined to official institutions with little academic content. In other words, the scientific networks were very limited, since any potential innovators were isolated, if not excluded; or else they hooked up with administrative or economic networks – which were hardly conducive to the capitalization of knowledge – rather than with academic networks.

These historical hypotheses – doubtless too summary and general, hence reductionist – merely aim to shed light on the work and careers of a few individuals who sought precisely to break with this general context, but whose work was not recovered and incorporated into a broader, more cumulative complex. Our purpose here is not so much to judge with hindsight than to seek to understand a history and a tradition (even if this means assessing, in conclusion, what remains of either). It is time now, therefore, to take temporary leave of a reductionist macrosociology and to examine in greater detail the work and efforts of a few statisticians and economists who can in various ways be compared to their Dutch counterparts described later[5].

[4] We use the past tense, for the French landscape-as broadly sketched out here-has undergone major changes in the past three decades. These would deserve another analysis, even though some aspects we discuss persist.

[5] Discussions on the internal history of French econometric models will be found in Artus, Deleau, and Malgrange (1986), Boyer (1987), and Courbis (1991). On the more general history of official statistics in France, see INSEE (1987), vols. 1 and 2.

The notion of quantifying the overall fluctuations of the economy – first by aggregating them and treating them as a whole, then by modeling the connections between them – was not self-evident. It was generally linked – more or less openly – to an action plan, to the idea that one can influence the course of economic life: Quesnay's Table is one of the earliest examples. The history of the quantification of social life illustrates this approach. The task is to identify and construct objects that hold together, in order to act upon them and adjust the planned action to circumstances. In the nineteenth century, "moral" statistics, then demography, developed around the political issues raised by epidemics, poverty, crime, and the size of the national population. At the century's close, between 1875 and 1895, the industrial nations were hit by a severe economic crisis. Wage-labor regulations were introduced; labor offices were founded in many countries in the 1880s (U.S.) and 1890s (Europe). In 1891, the Statistique Générale de la France (SGF), in operation since 1833, became affiliated with the national labor office (Office du Travail). The SGF launched surveys on wages and working conditions, followed by price surveys. New categories of perception of economic life – and of the means to act upon it – were formulated. They remained in use roughly from the 1880s to the 1940s. Their focus was on the cyclical recurrence of crises characterized by fluctuations in stock markets, wages, and commodity prices, by the production and circulation of precious metals, and by exchange rates between currencies. The economy was not yet perceived in terms of national production and of an integrated circuit of overall supply and demand, as it would be after Keynes (in theory), and Tinbergen and Frisch (in practice).

5. For Lenoir, the variable to be explained was the price

The observations by Juglar and the subsequent research by the statistician Marcel Lenoir were framed in terms of price and wage cycles. In 1913, Lenoir, an SGF staffer, defended a doctoral dissertation on "Price formation and movement of prices," whose originality and limits were symptomatic (Chaigneau and Le Gall 1998). The passage of an official statistician through the university circuit was an exception that would not be repeated for several decades. But from either point of view – that of his civil-service affiliation and the university – Lenoir's work was so atypical that it had no posterity on either side. It was virtually forgotten. No network, whether scientific or political, reclaimed it.

What did Lenoir do? Quite simply, he invented econometrics – without giving it that name. Significantly, his object of study is the movement of *prices*, not of production. Price is the variable to be explained: it was the focus of attention at the time. Lenoir sought to combine three of the eight approaches listed above – something no one before him had yet done. His theoretical analysis of price formation was inspired by contemporary mathematical economics (3.2). Walras, Pareto, Marshall, Edgeworth, and Jevons are mentioned. The procedures for adjusting between supply and demand functions are examined. But, unlike present-day practice in France, it is the *quantities* (supplied or demanded) that are plotted on the x-axis, whereas prices are shown on the y-axis. Prices are the endogenous variable to be explained, resulting from the trial-and-error interactions between supply and demand. This detail is a significant indication of the line of inquiry: the problematic issue is the price cycle. Moreover, Lenoir intuited that supply and demand curves shift from one period to the next, and that the path of their intersection is neither a supply function nor a demand function. However, depending on whether the goods are agricultural or industrial, the widest swings occur in the supply functions or the demand functions; as a result, according to the pattern, the intersection paths "resemble" supply functions or demand functions. Lenoir was thus on the trail of what eventually became the "identification problem" in econometrics.

In the second section of his book, Lenoir relates annual price series to consumption series, production series, and financial series, in order to analyze their changes and cycles (3.3). For this purpose, he uses the brand-new tools of mathematical statistics: multiple linear regression and partial correlation (3.4). Like Tinbergen twenty years later, he seeks the root cause of cycles in a theory, borrowed from Lexis and Aftalion, of the lag between capital spending and production:

> The root cause of the periodic fluctuations in economic life – in the theory to which we subscribe – is the intermittent demand for the fixed capital needed, from time to time, to refurbish and expand economic tooling (Lenoir 1913).

Lenoir also explains the long-term ("secular") variations in prices by the "production of numeraire," i.e., the production, circulation, and stocks of gold and silver. He successively analyzes monetary movements, the cycles of coal, wheat, cotton, and coffee, and the fluctuations in overall prices, measured by "index numbers." The innovative aspect is his attempt to associate formal theory, empirical series, and "econometric" regressions (the term "econometric" did not appear until 1930). Admittedly, the two parts of the book are not completely related. But if Lenoir does not actually evaluate supply and demand functions – something that the U.S. economist Moore was already trying to do (and Tinbergen was to accomplish in the 1920s) – it is no doubt because he has a clear intuition of the difficulty of the task due to the double translation of the theoretical curves from one period to the next.

Lenoir's research, which was pioneering in every respect, did not spawn significant developments. He made no allusion to its potential use as a guide for economic policy: such a claim would have been barely conceivable before 1914, especially from a young SGF statistician. After World War I, Lenoir was sent by the SGF to Hanoi to set up the Service Statistique de l'Indochine, and he died in Indochina in 1927. He was still occasionally mentioned by academic economists until the 1950s, then fell into total oblivion[6]. The SGF, for its part, did not support short-term economic analysis and forecasting, and its then director, Michel Huber, defended a highly administrative notion of the role of "official" statistics.

That same year, in 1927, the very young Jan Tinbergen joined the Central Statistical Bureau of the Netherlands, where he was to invent econometric modeling, and a Frenchman of the same age, Dessirier, joined the SGF. As "Economic Analysis Offices" were then doing in several countries, Dessirier sought to compile, publish, and prepare commentaries on economic series. But, fearing disapproval, he did not talk about his plans immediately with his boss. When he learned of Dessirier's plans, the SGF director urged him to resign if he wanted to continue his project. The minutes of a meeting of the Board of the Statistique Générale de la France of October 31, 1929 (a week after the Wall Street crash), recounts this episode in a manner that reveals the lack of legitimacy that such an activity still had at the time:

> ... Monsieur Dessirier has undertaken to publish a monthly collection of statistical curves entitled "current economic and financial conditions," containing assessments of the situation and forecasts. He admits that he started this publication in May 1929 and that he deliberately refrained from informing [SGF] about it, as he feared being barred from an enterprise to which he was deeply committed. Monsieur Huber [the Director] pointed out that this initiative seemed incompatible to him with Monsieur Dessirier's official position, and that serious inconvenience could result from economic and financial forecasts voiced by a civil servant on active duty, which might cause them to be regarded as almost official. The present format of the publication, which is covert and without publicity, cannot be maintained. Accordingly, the director should refer the matter to the minister, as he cannot take the responsibility for allowing Monsieur Dessirier to continue a private undertaking of this kind ... (Archives of the SGF Board, October 31, 1929)

6 André Marchal, in 1952, stated that he had died in World War I, which shows just how little known he was.

The minutes do not mention any discussion of the value or usefulness of a study of current economic trends. They are signed by the Chairman of the SGF Board, Clément Colson, professor of economics at the École Polytechnique, and Dugé de Bernonville, who authored the first estimates of French national income.

Another member of the SGF, Alfred Sauvy, was more prudent or more clever than Dessirier. In the 1930s, he started building a network in academia, politics, and journalism that gave credibility to – then demonstrated the need for – the construction, dissemination, and commentary on short-term economic series. In 1938, he founded an Institute of Short-Term Economic Analysis (Institut de Conjoncture) modeled on those of other countries. In twenty years, between 1930 and 1950, this activity became one of the key factors in establishing the legitimacy of the government statistical office. In 1946 – after the merger of several agencies, including the one in charge of short-term economic analysis – that office became INSEE, the National Institute for Statistics and Economic Studies. The first monthly sampling "Business confidence surveys" were carried out in the 1950's by André Piatier and Jacques Méraud (Monier 1987).

We can interpret this moment as a deliberate integration of some of the eight approaches in our list, whose synthesis is needed for econometric modeling. The notion of quantification (3.1) is advocated as an alternative to "literary" and verbal economics. But formalization in terms of mathematical economics (3.2) is lacking. By contrast, the sensitivity to changes recorded by time series (3.3) is central to the new synthesis. But regressions and correlations (3.4), national-accounting balances (3.5), and, *a fortiori,*, mathematically expressed economic dynamics (3.6) and probabilistic models 3.7) are totally lacking from the "short-term analysis" (conjoncturiste) approach-which also heavily emphasizes its usefulness as a guide for economic policy (3.8). The new method had its rationale, its language, and its audience. It was later criticized, in the 1950s, by the defenders of book-keeping consistency in "economic budgets," then, in the 1970s, by model-builders – in particular the developers of quarterly models (METRIC) – who were achieving a broad synthesis of the eight approaches.

The work needed to make the successful combination of the eight approaches technically feasible and politically credible was complicated and fraught with specific pitfalls in each country. In France, for example, a network of engineers contributed to this undertaking in the 1930s, to a degree and with an efficiency that remain in dispute. The X-Crise group, set up in 1930 by members of the École Polytechnique, aimed to serve as a forum for exchanging ideas on "rational solutions" to the economic and social crisis. It was not, strictly speaking, a locus of scientific research and innovation, but a place where one could circulate and connect ideas hitherto separately advocated in widely different settings (Armatte 1994). Dessirier and Sauvy edited a bulletin of short-term economic analysis (*bulletin de conjoncture*). Divisia described a conventional system of economic statistics, while Roy, Ullmo, and Gibrat began to report on the nascent field of econometrics. Marc Bloch and Simiand spoke on economic history. In February 1938, the Belgian Bernard Chait gave a paper entitled "The problem of economic crises," in which he outlined the dynamic model that Tinbergen had been developing for the Netherlands and the U.S. in the previous three years. Chait stressed the models' usefulness as a "basis for government decision-making." Tinbergen himself gave a talk in June 1938 on his "economic research on the importance of the stock market in the U.S." Speaking in French, he gave a vivid, pedagogic presentation of his concept of dynamic models, seen as mixed systems of differential equations and simultaneous finite-difference equations comprising as many variables as equations. He showed charts giving the results of the regressions and their interpretation. This type of methodology was not applied in France until more than thirty years later, in the early 1970s. The dissemination and applications of the ideas debated by the X-Crise group, and their actual impact, remain to be studied. We can see a discontinuity between these discussions of the 1930s on econometrics and the "French-style" national accounting that emerged a decade later.

6. Accounting balance or market dynamics

There is a striking difference between the course charted by Tinbergen's econometric modeling and the post-war French work on national accounting and economic budgets. On the one hand, the spontaneous fluctuations of an essentially market-based economy were monitored as closely as possible, and incorporated into a formal approach to reveal their endogenous dynamics and to test possible effects of specific economic-policy measures. On the other hand, the national economy was treated, in practice, more or less implicitly as the economy of a large corporation, whose internal flows needed to be analyzed in detail and organized so as to satisfy the variety of needs with the limited resources available. This approach is symbolized by the book-keeping method of balancing "resources" and "uses," with a cross-tabulation of balances by "agent" and by "transaction."

But it would be too simple to equate this distinction with the stereotyped opposition between "liberalism" and "dirigism," between left and right, between market and State. In the Netherlands of the 1930s, Tinbergen developed his cycle model and tested different economic-policy scenarios because, as an active member of the Socialist Party, he wanted the State to intervene judiciously in a serious crisis. By contrast, French policy in the 1940s and 1950s spurned the construction of a heavy, authoritarian planning system like that of Eastern Europe. Indeed, Jean Monnet skillfully crafted the Commissariat Général au Plan (CGP) as a light-weight structure, designed to organize consultations and dialog between government agencies and "social partners" (management and labor unions) who had previously ignored one other. This political line was, word for word, identical to that of the Central Planning Bureau (CPB) created by Tinbergen in that same year 1946. The bitter debates that preceded the two births were identical and ended in the same way in both France and the Netherlands: they pitted the supporters of "heavy-weight" planning that set direct guidelines for economic policy against the advocates (such as Monnet and Tinbergen) of "light-weight" consultative planning. From this common starting point, however, the two histories diverged. In the Netherlands, after a brief trial-and-error period, model construction resumed in the early 1950s, and the models were to exert a considerable influence on the social debate for years to come (Barten 1991). In France, by contrast, the construction of the national accounting system became a vast political and administrative undertaking, but did not generate substantial models until two decades later. Moreover, the influence and authority (in academic and political terms) of the French Plan rarely matched that of its Dutch counterpart. This shows that a simplistic opposition between "French-style" Statism and "Dutch-style" economic liberalism fails to account for the differences between the two histories.

In most of the leading countries, the design and implementation of a national accounting system (and the necessary statistical infrastructure) were treated as technical operations – vital and costly, but nevertheless of secondary political and academic importance. The available histories of macroeconomic modeling do not feel the need to discuss this phase of the work. Statistical data exist in the same way as roads and telephones. Their non-existence is an index of under-development. In France, by contrast, this episode in the construction of the economic information system was experienced in two ways in the approximately twenty years from 1950 to 1970 or so: first, as a tool that soon came into routine use; second, as the vector of a much bigger political plan to rationalize and modernize French government[7]. The venture was, of course, marked by the distinctive post-war conditions, which were in fact common to France and the Netherlands: reconstruction; investment in infrastructure partly financed by a Marshall plan dependent on the existence of an economic accounting system; shortages; inflationary situation contrasting with that of the 1930s; persistence of heavy red tape; and decolonization (Indochina and Algeria for France, Indonesia for the Netherlands).

[7] For a lively account, based on interviews with the key players, see Fourquet 1980.

The specific feature of the French experience was the transformation of the State's role, accomplished *from within* by a team of senior civil servants. Although they wanted to modernize the administrative system, they were the product of the old structure described above, which assigned greater legitimacy to the State's internal expertise than to a more academic form of expertise. The intellectual matrix of French national accounting was a combination of (1) the book-keeping and budgeting tradition of the Finance Ministry bureaucracy, extended to all economic players, and (2) a simplified version of Keynesianism, stressing the numerical gap between total demand and total supply measured with precision by the new tool. Paradoxically, this enterprise led to an elaborate system of accounts far more detailed and sophisticated than those of other countries, but less used in economic models drawing on recent developments in econometrics and macroeconomics. In sum, *the sophistication of the French system served a purpose other than modeling*.

Indeed, how do we explain this apparently limited use, by comparison with the standards being introduced elsewhere in the same period? To understand this, we need to delve into the internal mechanisms of the national-accounting process and of the short- and medium-term forecasts derived from the process. The national-account tables offer a subtle, consistent, and comprehensive theoretical description of all economic flows between agents. As the statistical sources available are spotty, incomplete, and contradictory, filling the tables is a collective process consisting of successive approximations, through trial-and-error, negotiations, and compromises between the persons responsible for each line and column of the accounting matrices. In a way, the process mimics (or thinks it is mimicking) the real functioning of an economy perceived as a series of book-keeping adjustments and not as a dynamic market-based mechanism. Significantly, projections by *volume* are central to the reconstructed past series and to short- and medium-range forecasts. Prices and values are, of course, observed and recorded in the accounts of the past, but their rationales remain, in part, mysterious.

7. The future and the past are built in the same manner

This *modus operandi* had another consequence, which doubtless explains why a formalized econometric modeling of short- and medium-term forecasts was not introduced in France at that time, whereas it had been developed in the Netherlands by the early 1950s. The task of constructing the accounts of the past, present (current year), and future was being performed by the same people, on a continuous basis, under ultimately similar constraints: this may come as a surprise, to the extent that the two tasks – forecasting and national accounting – so apparently different in nature, ought to be conducted by separate institutions (in the Netherlands, CBS and CPB). Such a division of labor is more consistent with a realistic epistemology, in which statisticians would measure past flows and lock them up in black boxes – data bases made available to econometricians and forecasters. The discontinuity between the statuses of the two sets of numbers would thus be clear.

But that is not how things were done in France. The accounts of the past, in any event, require trial-and-error and negotiated evaluations. Moreover, in each year, they are reworked several times in response to the constant inflow of new information. The recalculations are performed by projection (in volume and price terms) from the previous year's accounts, which are also in a state of continuous revision. In turn, the accounts of a relatively recent past are used to assess the accounts of both the current year and the near future (economic budgets). In other words, there is a series of about five or six accounts (from t - 3 to t + 2) that are under ongoing construction; they are modified using ultimately similar procedures for the past and future, in the light of information or partial "forecasts" concerning a given portion of the table. The difference between past and future is thus blurred: there is a continuous shuttling between future and present, then to the past – with specialists constantly on the look-out for the twists and turns in an economic life already framed and scripted by "economic budgets." These are, in turn, cited in economic-policy discussions in government or in the press, which helps endow them with a status of reality and increases their convergence with the accounts of the past[8].

This relative indistinction between the two categories of accounts has naturally been debated. Some participants did not fail to denounce the ambiguity of these "accounts of the future," both descriptive and normative, which extrapolated "objective" trends and at the same time were the outcome of deliberate policies. Some parts of these accounts (in particular *price* movements) were strongly suspected of being performance-oriented and "unrealistic" (announcing sharp rises in advance is enough to trigger them). This debate has recurred ever since national accounts and economic budgets were invented. In 1962, however, the most institutional aspect of the ambiguity was dissipated: in principle, past accounts were henceforth to be compiled at INSEE; those of the future, in another government office (the Finance Ministry's Forecasting Office: Direction de la Prévision). In practice, however, the teams stayed close, and the continuity between past and future accounts held firm, at least until the 1970s. Then, computerized econometric models, first annual then quarterly, diminished the importance of the direct (and partly informal) negotiations between people who knew each other well – a phenomenon that underlay the construction of future accounts and past accounts alike (Kramarz 1989).

The logic and consistency of this set of procedures cannot be understood unless we relate them to their origins and institutional introduction. The term "economic budget," which denotes what the Dutch call "short-term planning," effectively symbolizes the purpose of this enterprise. Just as a company or the State compiles *accounts* of its past revenues and expenditures (which must necessarily balance) in order to assess the *budget forecast* for its future revenues and expenditures, so the entire nation sets up an accounting system to record all the flows of exchanges between its main categories of players, in order to prepare a forward-looking "economic budget" in the same framework. The chief aim of this "budget" is to provide economic evidence and justification for the "central-government budget," i.e., the revenues and expenditures of a specific player. In the initial period, therefore, French national accountants were entirely focused on those same goals of book-balancing and fiscal equilibrium. While they were effectively striving to incorporate budget decisions into a macroeconomic-equilibrium framework, the equilibrium did not fit into an endogenous dynamic path, as in the Dutch models based on Tinbergen's 1936 model.

Yet the notion that filling the theoretical cells of the forecasting tables could be helped by "modeling" occurred very early. The inspiration and leadership for the operation came from Claude Gruson, who in 1950 proposed a theoretical model comprising 88 equations, of which 11 accounting identities. Seeking to reveal an "inflation gap," this complicated model remained virtual and was never quantified. In the fifteen years that followed, the tools used by forecasters were not systematic econometric relationships, but constraints and relationships reflecting a "basic Keynesian" logic that linked overall supply and demand, *on a case-by-case basis*. This strenuous collective effort to compile accounts of the past and future simultaneously was carried out by a new office of the Finance Ministry created by Claude Gruson in 1950: the Office of Economic and Financial Studies (Service des Études Économiques et Financières: SEEF). The Office was separate from INSEE. The latter, founded in 1946, was the successor of the Statistique Générale de France (SGF) and the Institut de Conjoncture set up by Alfred Sauvy in 1938. In 1962, Gruson became head of INSEE. SEEF – the breeding-ground of national accounting – was split into two units. The first, responsible for the accounts of the past, was incorporated into INSEE, turning the Institute into the producer of the national accounts. The second became the Ministry's "Forecasting Directorate" (Direction de la Prévision).

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8 The national accountants of that period believed that if the accounts of the recent past were known in detail, the forecasting of accounts in the near future would become almost child's play. This notion resembles that of Quetelet, who boasted that he could make predict the exact number of suicides and crimes that would be committed the following year. In this connection, he even talked about the "inexorable budget of crime."

Between the 1950s and 1970s, "economic forecasting" was conducted in parallel, in two different and partially competing ways. The "budget people" (*budgétistes*) at SEEF incorporated annual forecasts in the accounting framework described above, while the "short-term economic analysts" (*conjoncturistes*) at INSEE carried on the old tradition of Dessirier and Sauvy. They constructed intra-year series (monthly or quarterly), then organized specific business surveys (*enquêtes de conjoncture*) among executives. The INSEE analysts thus cultivated an intuitive approach to current trends, based on information items that were admittedly quantitative but not formally interlinked; their methods and language were very different from those of the "budget people." These approaches typically correspond to two of the points in our list: 3.3 for the forecasters, 3.5 for the national accountants. At the time, the Dutch began a synthesis between 3.3 and 3.5, with the aid of tools from 3.4 (mathematical statistics) and 3.6 (dynamic modeling).

Despite their long-running quarrels, the common feature of the French "budget people" and "forecasters" – by comparison with the Dutch – is that they do not postulate long-term regularities embedded in economic laws and econometric relationships. A similar opposition is described by Mary Morgan (1990) in her "history of econometric ideas." She divides the students of "business cycles" into two opposing two groups: *economist-statisticians*, who see each cycle as unique, with its specific life-course and logic (two typical representatives are Mitchell and Burns at the NBER), and *econometricians*, who seek to isolate regular patterns and atemporal laws, first using exogenous cycles (Jevons and Moore, with their astronomical and climatic explanations), then by means of an endogenous dynamic linked to market adjustments (Tinbergen). On these criteria, the forecasters and national-accounting "budget people" belong to the first tradition (Mitchell was one of the founders of U.S. national accounting).

The oppositions are not, of course, so clearcut. For example, French forecasters long justified the absence of econometric models comparable to those of Tinbergen and of Klein and Goldberger on the grounds that there were no statistical series long enough to feed them. They stressed the urgency and importance of massive methodological and statistical investment before the search for possible regularities could begin. While true, this explanation may not be entirely adequate: models relying on short, approximative series have been constructed elsewhere. More profoundly, the French approach seems closely linked to the partly common positions and roles of SEEF national accountants and INSEE forecasters in the government bureaucracy. Being close to fiscal decision-making or the immediate interpretation of the economic "climate," they are inclined to emphasize the specificity of the present, or rather that of a perception immersed in action-as against the broader perspective adopted by observers (for example, academics) who can detach themselves from the contingencies of that present. Even if, like Tinbergen, the detached analyst performs a service requested by the political sphere, he or she maintains an academic behavior, and writes articles and books of wider import than administrative memoranda. The particularity of "French-style" expertise is very obvious here. The main players in national accounting were essentially engineers who had become senior civil servants. They wrote little, or produced "methodology volumes" for direct operational use.

8. Pulsation and planning: two philosophies of time

The above distinction between senior civil servants and academics does not, however, overlap an apparently similar opposition between the short term (the politician prisoner of emergencies) and the long term (the academic specialist who reputedly takes the longer view). "Taking long-term requirements into account" was actually a leitmotiv of the group of post-war planners and modernizers in the French civil service. The creation of the Plan was specifically aimed at satisfying a need that "government departments in charge of day-to-day matters" could not meet. There is a seeming contradiction between the stated desire to place short-term action in a long-range perspective and the refusal to embed temporality in the regular patterns revealed by econometrics. But are we dealing with the same "long" time? Is the cyclical time produced by the endogenous dynamics of the market economy comparable with the "long term" of the "prospective" approach? Cyclical time is the "substrate" for specific movements comparable to the pulsations of a mechanical or biological system. The long term is the distinctive action space staked out by the experts in "structural trends," who believe in shaping change. However forcefully the existence of such trends is asserted, they are incorporated into balanced tables reflecting the long-term effect of book-keeping constraints, but they do not result from a specific temporal dynamic of mutual, lagged causes and effects. While cyclical-fluctuation time is typical of market economies, the time scale used in long-term project planning can characterize economies of very different kinds, once rational-minded agents prepare accounts of resources and their uses. These two approaches rest on two different philosophies of time.

The comparison between the French and Dutch plans reflects the differences outlined above. The goals of the French plan are recorded in the national accounting tables; they are framed in terms of an overall consistency between extrapolated equilibria and willful initiatives, for a horizon year. By contrast, the Dutch plan incorporated, econometric formulations of spontaneous trends in a market economy as early as the 1950s, notably via "sliding plans" that connected short-term forecasts with medium-term forecasts. This approach effectively showed the long term to be as much the consequence of the summation of short time periods as the horizon for pre-assigned targets.

Yet, at their origin, the two plans were informed by similar political goals, partly due to the 1930s crisis of democracy. The aim was to invent a consensus-building technique based on a system that partly differed from conventional parliamentary representation, but was complementary to it and engaged in dialog with it. On this count, the Dutch seem to have gone further and their planning has undoubtedly had a deeper impact on the democratic debate[9] than was the case in France, at least in the long run. In both countries, *ad hoc* commissions of experts appointed by the main business organizations, labor unions, and (in the Netherlands) religious bodies began iterative interaction with a team of technical experts whose role was to verify the consistency and economic consequences of each player's plans and demands. The administrative workings of these two new machineries were not strictly identical, but the philosophy was.

The two undertakings differed, however, in the *technical procedure for verifying the consistency of the plans*. The French prepared a detailed, complex framework of double book-keeping balances – by agent and by transaction – that was projected onto the Plan's horizon year. Its detailed breakdown was supposed to guarantee the plausibility of the overall enterprise. This total consistency was the outcome of iterations between the technical experts responsible for the calculations and the specialized commissions, each a forum for important but partial points of view. The technical process of filling the tables for the horizon year (t + 5) closely resembled the one described above for compiling accounts of the past and near future (the economic budgets). The alchemy was the same: it involved transforming partial, contradictory information into matrices exhibiting line-and-column balances, without worrying about the dynamic path leading to that horizon. The social consultative procedure, meanwhile, sought to promote not only the overall equilibrium of the future accounts, but also a political pedagogy: this consisted in comparing points of view and plans in a now common language – in other words, the establishment of a new type of public space that differed from elected representation.

9 One of the topics most actively discussed and explored in the period was wage policy (Hessel 1965).

9. Political model and econometric model

In the Netherlands, the overall political enterprise was the same, but it was based on a very distinctive tradition – dating back to the mid-nineteenth century – of distributing and balancing power between the religious and political communities: this politics of "pillarization" (*verzuiling*) authorized and organized a strict, fair division of social life (marriages, schools, labor unions, social protection, press, radio, and political parties) between Protestants, Catholics, Liberals, and Socialists (De Voogd 1992). The system flourished until the 1970s and informed the Plan's creation. Indeed, the notion that these communities could lead partly separate lives but also get along and jointly form the "pillars" of a State that would guarantee them autonomy and civil peace was deeply rooted in the Dutch political tradition[10]. For instance, the Social and Economic Council, in charge of discussing the results of CPB forecasts and estimates, is composed of qualified representatives of each "pillar" appointed by specific labor unions, employers' organizations, and community groups. The Plan's neutrality and objectivity are guaranteed by this representation.

On this criterion, the Dutch State seems less omnipresent and sovereign than the French State[11]. This may perhaps shed light on the differences between the procedures and tools used for interactions between technical experts and representatives of social forces. The Netherlands was able to use an econometric construct of academic origin – sophisticated in terms of both economic analysis and mathematical formalism – as a legitimate, accepted medium for the interactions. France, by contrast, used a tool for book-keeping consistency of a more administrative kind. Developed in the most solid core element of the French State (the Finance Ministry, Rue de Rivoli), it provided the necessary legitimacy with little recourse to academic language. Thus a different relative equilibrium between the State and other forces (universities, labor unions, churches, etc.) can shed light on the existence and success of tools that are both technical and political – tools whose definition cannot be reduced to a list of the model's equations, even if these need to be incorporated into the analysis.

This situation is illustrated by Jan Tinbergen's atypical career, which was an essential feature of the Dutch academic and political constellation. Born in 1903, Tinbergen was the son of a Dutch teacher, who belonged to a very small Protestant denomination. He studied physics at Leiden in the 1920s, in a scientific environment that included some of the best physicists of the period[12]. In 1929, on the advice of his teacher, Ehrenfest, he defended a doctoral thesis on "the problems of minima in physics and economics," in which he drew on mathematical formalisms used in physics (Hamiltonians, Euler equations, and differentiable dynamic systems) to deal with the dynamics of a Walrasian economic equilibrium based on maximizations of utility or profit (Boumans 1992). But he did not want to embark on a career in theoretical physics because, in his words, "the problems of society seem more urgent to [him]." Moved by the plight of workers in the Leiden

10 This arrangement rested on two partly different theological traditions. For the Catholics, the notion of *subsidiarity*, derived from the papal encyclicals, guaranteed a division of competencies between the levels of a hierarchy: the State had sole (but complete) responsibility for performing the tasks that the lower ranks could not carry out. For the Protestants, the notion of *sovereignty in one's community* gave more prerogatives to small communities and expressed greater reservations toward the State. For this reason, the Dutch Plan was promoted more energetically by alliances between Catholics and Socialists, although Protestants and Liberals also played an active role (van Altena, van den Bogaard and Kalshoven 1998). One would surely find similar distinctions in the history of European integration, marked by the figure of Jean Monnet, the founder of the French Plan.

11 Its capital, The Hague, is a small town by comparison with Amsterdam or Rotterdam. The CBS and CPB are located in The Hague or its environs. (The CBS building, in Voorburg, is also the seat of the permanent secretariat of the International Institute of Statistics, IIS). Tinbergen lived in The Hague. The main universities are distributed between Amsterdam and three other cities: Utrecht, Leiden, and Rotterdam. According to Johan Heilbron (1988): "This polycentric system, based on coexistence rather than open competition, goes back to the origins of the nation, which was formed by the union of the provinces, without a commanding center, prestigious court, or central authority."

12 Following a secondary-school reform of the 1860s that promoted scientific education, well-endowed physics laboratories flourished in the early twentieth century, and the Netherlands won five Nobel prizes in physics between 1901 and 1913 (Willink 1991). One laureate, Paul Ehrenfest (1880-1933), a father of quantum mechanics, had a decisive influence on the young Tinbergen.

region, he joined the Socialist Party, in which he remained active all his life. A pacifist, he applied for conscientious-objector status in 1927 so as to be assigned to civilian duty. His father secured him a place at the Central Bureau of Statistics (CBS), where he eventually worked until 1945, while teaching economics at Rotterdam University and carrying out missions for the League of Nations.

Tinbergen had thus studied the most advanced mathematical economics of his time and even contributed to its development. His contact with empirical data made him want to use them to address tough social issues. The conditions for the potential grouping of approaches 3.2 (general equilibrium), 3.3 (short-term analysis), 3.4 (mathematical statistics), 3.6 (dynamics), and 3.8 (political goal) – needed to give birth to econometric modeling – were assembled. Two other components, national accounting (3.5) and probabilities (3.7), were still missing but would be added a short while later. Tinbergen's modus operandi bears some resemblance to that of Lenoir – also a member of the official statistical corps – but it goes much further. He not only raised the issue of identifying supply and demand functions; he was also practically the first to solve it – in an article published in German in 1930 – by adding more explanatory variables and calculating a reduced form (Morgan 1990). He applied this method concretely to the market for potato starch, an important commodity in the Dutch economy.

Tinbergen then turned to the study of "spider's web" movements that generated market equilibria. This led him to the structure of the first dynamic model of the business cycle, which he formulated, estimated, and presented in public in 1936 (Boumans 1992). His work was in answer to a request from the Socialist Party. What measure is most likely to revive economic activity and restore the country's trade balance? A public-works program, protectionist measures, an industrial-rationalization policy, a reduction in monopoly prices, wage cuts, or a devaluation of the guilder? Tinbergen tested these alternatives. He also studied the effects of measures aimed at counteracting an imported cycle through exchange-rate management or public investment. He concluded that by far the most effective measure was devaluation, which the government decided on shortly after.

This study is little known outside the Netherlands, but Tinbergen was later asked by the League of Nations to test the cycle theories compiled by Haberler. For the project, he worked hard to prepare a clear, educational presentation of his model by stripping it of its most arid mathematical formalism while preserving its essence: the combination of an endogenous dynamic of market mechanisms and the possibility of influencing these by measures whose effects are analyzed within the logical framework of that dynamic itself. The big issue at the time was the "stabilization of the business cycle." Do stabilization measures actually diminish economic activity even further, through the chain of repercussions that can be predicted from the cyclical structure of the model's core? This question is discussed in detail in a book written and published in French in Paris in 1938, *The Mathematical Foundations of the Stabilization of Business Movements* (*Les fondements mathématiques de la stabilisation du mouvement des affaires*), in a series on "theoretical economics and economic statistics" edited by Georges Lutfalla[13].

The construction and early experimental uses of Tinbergen's model in the late 1930s could rely only on rudimentary data. In 1937, this situation prompted CBS to undertake the establishment of a permanent system of national accounts (or, as it was then called, "national income"), focused on income statistics and corroborated by production statistics (Derksen, CBS). The war economy and the ensuing reconstruction quickened this research program during the 1940s in the Netherlands, as it did in France and elsewhere. An organized economy facing shortages, emergencies, and an intensive, concentrated deployment of resources requires direct intervention by the State and government agencies-hence the introduction of new statistical systems,

13 Unless further research proves otherwise, the book seems to have been little read or quoted by the French economists who created national accounting and economic budgets. Malinvaud remembers having bought it and read it closely in 1943.

particularly to document industrial production. This happened in both the Netherlands and France. During such periods, the notion of an independently functioning market economy with its own cycles and tempo – explored by Tinbergen in the 1930s – gave way to the vision of an economy managed like a big corporation. Within this shortage economy, an increasing number of manufactured goods circulate between workshops (the "industrial branches") interlinked via the "technical coefficients" of a Leontief matrix. Prices do not seem to act as key variables, as in market transactions.

10. Is the economy comparable to a large corporation?

The notion that the overall economy resembles a "single big enterprise" was expounded by the first producers of national accounts. In 1941, the Dutch statistician Van Cleeff proposed the expression "book-keeping," which remained in use in the Netherlands for many years, to emphasize the analogy with standard business accounting. And he stressed the importance of such a tool for a "centrally planned economy" (Bakker 1992). In 1943, the Frenchman Louis-André Vincent – one of the precursors of French national accounting – articulated the same analogy between the entire economy and a big company (Fourquet 1980), which persisted among post-war national accountants. But it then gradually drifted from a literal meaning, still very plausible in the 1940s context, toward a more metaphoric meaning. Managing the economy like a single large enterprise, Soviet style, was no longer on the agenda. However, the image continued more or less explicitly to inform the vision of the founders of the major systems of national accounts. The tendency was all the stronger as the founders were close to the core of the State, i.e., the place where it is most plausible to conceptualize the economy as a whole. At the time, however, Keynesian language, in its simplified version, was better suited than the language of the Taylorist rationalizer for describing the actions that could be envisaged in the context of an economy freed from its wartime regulations – an economy where the notion of macro-economic steering was now accepted. The tool modeled on the detailed book-keeping of an industrial firm thus gradually evolved into a system of more general estimates required by economy ministers.

However, the evidence suggests that this transition, which began from comparable situations, followed different paths in the Netherlands and France – notably under Tinbergen's influence. In the immediate aftermath of the war, the Dutch plan had to deal with rationing and the urgent problems of reconstruction. By 1953, the planners, almost all of whom had been trained by Tinbergen, sought to build a model that could reflect the movement of the economy. But this initial model was not as dynamic as its 1936 precursor: its only dynamic part was the investment equation, based on the mechanics of the flexible accelerator (Central Planning Bureau 1956; Barten 1991)[14]. By the 1960s, however, the "sliding plans" system, which linked the short – and medium-term plans together, reintroduced the idea of an endogenous movement of the economy: the economy's course could be influenced by economic-policy measures tested by the model, whose results were widely disseminated and discussed.

The French Plan was informed by a partly different philosophy. It held on longer, at least implicitly, to the metaphor of the large corporation (with emphasis on the importance of projections in volume terms, Leontief tables, and technical coefficients). At the same time, it combined the metaphor with a more political goal of attenuating market effects by a sort of consultative simulation of the market through the planning commissions. In these conditions, as we have seen, national accounting was the ideal tool for achieving consistent, collectively transparent plans. By contrast, macroeconomic modeling – with its mysterious and automatic

14 Some of the Plan's originators-particularly van Cleeff, who created the first national accounts in 1941-may have had a different vision than Tinbergen's; they may have wanted a Plan less centered on the market and more focused on a sort of communitarian socialism. By his sensitivity steeped in religious thought, van Cleeff resembles Gruson (an active, influential member of the French Reformed Church). Unlike Gruson, however, van Cleeff did not play a major role after 1950 (these comments owe much to Frank Kalshoven and Adrienne van den Bogaard).

hypotheses and regressions – would deprive the social partners of their capacity to discuss an economy clearly reflected in a detailed, efficient system of statistical information. This concept of planning is expounded with near-prophetic vigor in Claude Gruson's many writings on the topic since the 1950s (for example, Gruson 1968).

In 1963, the Brookings Institution in Washington held a conference on "quantitative planning of economic policy" (Hickman 1965). France was represented by a member of its Planning Commission (CGP), Bernard Cazes; the speakers from the Netherlands were Henri Theil, an econometrician in the Tinbergen tradition, C.A. Van den Beld, of CPB, and Willem Hessel, a labor-union expert and member of the Economic and Social Council. Cazes confined his talk to the French Plan's institutional organization and the consultative procedures between commissions and technical experts. All the other speakers – including, of course, the Dutch – presented models. French planners at the time explained the difference by the fact that (in their view) France was the only country with an actual planning procedure, whereas the other countries could only "model in the abstract." While no doubt accurate for the other countries, this analysis was incorrect for the Netherlands, which combined the two procedures, political and econometric[15].

French distinctiveness faded by the late 1960s, when it began implementing the FIFI model followed by the DMS model (Boyer 1987; Courbis 1991). The FIFI model, however, was still limited to a final-year forecast (*année horizon*); its main specific feature was its distinction between an "exposed sector," for which prices are exogenous and determined by the rest of the world, and a "sheltered sector," where a classical supply-demand equilibrium remained possible. By contrast, the DMS model (acronym for "multi-sector dynamics": *dynamique multisectorielle*, 1975) was explicitly dynamic. In those years, however, the initial institutional mechanism of the planning commissions gradually lost importance. Attempts were made in the 1970s to use the FIFI and DMS models (in that order) to test alternative scenarios at the request of the "social partners," but the procedure never assumed the scope and resonance that it has displayed in the Netherlands since the 1950s.

Econometrics as a mathematical tool for economic analysis was introduced in France by economists trained first as engineers then as statisticians. Their intellectual profile differed both from that of academic economists – who long resisted the formalization of economics – and from that of the planners and national accountants we have just discussed. René Roy, an *ingénieur des ponts et chaussées* (i.e. trained in bridge and road engineering), studied and estimated consumption functions as early as the 1930s and was the first to train students in the method (given that Lenoir had gone virtually unnoticed since his 1913 research). After Roy, Edmond Malinvaud – back from a visit to the U.S. Cowles Commission in 1950 – introduced and reformulated probabilistic econometrics in France and trained generations of econometricians, in particular at the INSEE-run École Nationale de la Statistique et de l'Administration Économique (ENSAE). An academic more than a political person – in contrast to Claude Gruson – Malinvaud was not driven by the same motives as, for example, Tinbergen in the 1930s. His priority was to train specialists who would be familiar with the latest developments, and who would maintain close contacts with research in the English-speaking world. In this respect as well, the economists trained by Malinvaud differed sharply from the traditional academics and the planners. In the 1970s, this new economic culture began to spread in the French universities, drawing French economics closer to that of other countries including the Netherlands. In the Netherlands, econometrics loomed large in the academic curricula since the 1950s, notably though the influence of specialists such as Tinbergen, Theil, and Koopmans.

15 A third country follows planning procedures that may be compared to that of the Netherlands and France: Japan (Hickman 1965).

11. Optimization or self-fulfilling prophecy

The specific configuration in the Netherlands explains why the country witnessed, very early on, a close connection between a high-level econometric investment and the formalized expression of economic-policy goals. This combination was already at the core of Tinbergen's initial project. It continued, after the war, with the investigations by Tinbergen and his student Henri Theil on economic-policy optimization (Hughes Hallett 1989). Their approaches were partly different. Tinbergen assumed that the political decision-maker specifies the values to be reached by the "objective" variables of economic policy ("fixed targets"). The model makes it possible to determine the values of the action variables ("instruments") compatible with the chosen targets. The decision-maker can choose among these solutions, if they exist. Theil, by contrast, does not start with predetermined fixed targets. He specifies a decision-maker preference function that includes both the target values and the instrument values, without setting either in advance. The function is maximized under the constraints expressed by the model's relationships. The inclusion of the action variables in the decision-maker's utility function means that targets cannot be totally separated from instruments. Thus public expenditures may be regarded at the same time as instruments (in a Keynesian vision where they contribute to total demand) and as targets (to the extent that they are useful to social welfare) (Hickman 1965). The two approaches, in terms of fixed targets or preference functions, require a complete econometric model comprising targets and action variables. In the early 1960s, the Dutch CPB used an annual model of this kind with 36 equations, designed to support both forecasting and a short-term stabilization policy (referred to as "short-term planning") – following Tinbergen rather than Theil. The aim here was to use the model, combined with information on political decision-makers' preferences, to delineate a set of possible government actions capable of reaching numerically specified targets.

The French Plan – which reached the apogee of its glory in the 1960s as well, under the leadership of Pierre Massé – was guided by a different political and intellectual agenda. The goal was to set a medium-term growth slope compatible with human and technical resources, and with social structures. The prime concern of this willful enterprise was to choose a growth rate. The maximum horizon was defined by a "Japanese-style growth" scenario: this was the planner's dream, but was deemed too violent because of the upheaval in social structures that it would require. Empirical sociology – then in its infancy – was needed to analyze "resistances to social change" that inhibited growth. The planning exercise was a socio-political process in which individual projects were set out in *ad hoc* commissions, coordinated through the national-accounting balances, and gradually adjusted to comply with the book-keeping, economic, and social constraints. The basic idea was that the exercise would convince the social players that the proposed growth target was possible and plausible. The prophecy was self-fulfilling as it did not become reality unless people believed in it beforehand. This vision was the key to the deliberate rationalization of growth that the French national accountants sought to achieve.

The then Planning Commissioner was Pierre Massé, *ingénieur des mines* and an economist familiar with research in other countries. Under his impetus, several pilot optimization models were constructed in the 1960s, notably by a center for mathematical-economics research set up by the planning authorities: CEPREMAP (Moustacchi 1965). Unlike the work of Tinbergen and Theil, however, these projects were not incorporated into policy-making; they remained textbook exercises. The models actually built and used later on – ZOGOL, DECA, STAR, and COPAIN for economic budgets; FIFI and later DMS for medium-term planning – were in fact increasingly detailed and sophisticated formalizations of forecasting practices previously conducted through trial-and-error and iterative negotiations. These trend projections, sometimes diversified into "scenarios" of different shades of "pink" or "gray" depending on the growth rate, could also serve as the basis for economic-policy simulations (*variantes*) at the request of political or labor leaders. But this "customized" simulation never assumed major proportions.

Each approach had distinct implications in terms of negotiation and construction of socio-political networks. In the Netherlands, the concrete expression of the goals of decision-makers' "preference functions" is achieved in the discussions at the Economic and Social Council, where employers and employees are represented. In the early 1960s, for example, a heated debate took place on alternative wage policies. At the time, wages were surging in what was viewed as an inflationary pattern. One labor-union participant noted the extent to which "preference specification is essentially a negotiation process, subject to the vicissitudes of politics, in which the commission members' preferences interact without necessarily converging" (Hessel 1965). This issue of the specific effect of the negotiation dynamics on the formulation of political goals was clearly perceived by the Dutch planners. Their French colleagues preferred to emphasize the scope and intricacy of the vast network of government departments, commissions, and working parties that participated in the gradual elaboration of the Plan, which was then discussed and approved by Parliament. For them, it is the extension of this involvement by social players that makes the Plan more credible, since a greater number of players are incited to act in conformity with a collectively defined horizon. In either case, the style of the technical formulation – optimizing and econometric for the Dutch, coordinative and accounting-based for the French – interacts with the academic, intellectual, and social network into which the formalization is incorporated.

This procedural difference may explain why Dutch planning seems to have shown greater resilience than its French counterpart in the turning-point of the 1970s and the advent of global market crises. In the Netherlands, the technical tool proposed by the modelers encountered few objections. Each of the main political parties agreed to elaborate its own proposals within the planning framework, which it accepted as an "arbiter." The debates centered on the expression of political priorities, under the new constraints introduced by a situation partly resembling the one modeled by Tinbergen in the 1930s. The market's broadly cyclical fluctuations could now be analyzed and predicted. The impact of price mechanisms on income distribution and employment was taken into account, in the model language, by the political players – just as the religious and philosophical "pillars" formerly cooperated in *verzuiling*. The model was thus integrated into a superstructure overarching pillars that remained separate but were willing to discuss under these rules.

In France, by contrast, the collective consultation process, originally designed to discuss the medium-term growth rate measured by volume, was no longer suited for tackling issues that now lacked a common language – issues raised by market mechanisms and the employment crisis. Modeling, which flourished and was diversified in various institutions founded around 1980, has become an exercise involving technical experts far more competent in econometrics and economic theory than their predecessors of the 1950s and 1960s. But their work is harder to incorporate into a collective social practice, as the issues raised are more controversial and harder to discuss in a neutral and relatively dispassionate forum such as the commissions of yesterday. Moreover, the very language of models and economic theory is no longer a common ground broadly accepted by all. Once again, therefore, we have a separation between technical experts using ever more sophisticated tools and a social debate that partly ignores them, or is perplexed by the diversity of the experts' diagnoses and proposals[16]. For example, the arguments in the controversies over the minimum wage seem to have remained frozen for the past sixty years, at least in France. A close comparison of the style and social impact of this chronic debate in two countries that resemble each other in so many ways would be instructive. How have the two planning and modeling traditions shaped the connections between model-backed arguments and policy-making? Is there a significant difference between the Netherlands and France on this score? Indeed, do the words *plan* and *model* have the same connotations depending on whether they describe things to do, things that are being done, or things that have been done – in either country?

[16] This rough comparison between the two countries is largely a hypothesis. It may seem simplistic-especially for the Dutch, who, as insiders to their situation, perceive its contradictions better; perhaps they will see the consensus described here as their national utopia.

12. The Plan as project and picture

The concept of *plan* is two-faceted. First, the plan organizes and coordinates a sequence of actions for a given purpose. Second, the plan is a graphic representation, a fence around a complicated and potentially unlimited space, reduced to the sole features needed to move inside that space and act upon it. The concept of *model* has the same ambivalence: an exemplary figure to which one should aspire versus a conventional, manipulable stylization of a system of interactions. In both cases, the possibility of viewing and describing and the possibility of acting are closely combined (Thévenot 1990). However, at least in their economics-related uses, the term *model* appears to lie on the side of description, whereas the term *plan* seems linked to an action that is being contemplated. This distinction may perhaps enable us to summarize what we have said earlier by separating the course of the economy itself from the course of the activity involved in planning that economy.

The Dutch rely on the dynamic sequence of fluctuations of an autonomous market economy, as one would try to mount a galloping horse. The designation of fixed or flexible targets is conditioned by a keen sensitivity to the movement of the economy. Meanwhile, the planning procedure implies a close link between modeling and the negotiation of targets and instruments. The emphasis seems to be on the procedure's outcome. *The Dutch Plan mimics the economy's course by its model*.

The French (before 1975) had a more technical and quantitative vision of an economy whose price dynamics long remained a mystery. Whereas the economy's movement was summed up by its horizon, the emphasis was on the social procedure for defining the horizon in detail, via a complicated circuit involving national-accounts experts, commissions, and working parties. *The French Plan mimicked the economy's course by the negotiation between social partners*.

One should not, however, expect too much from these macrosociological comparisons. Their main purpose is to underscore the conventional descriptions of individual countries. Comparisons, whether international or historical, enable us to *see* by giving us perspective. But from that point on, everything remains to be done. The microsociological study of interactions between academics, statisticians, delegates of French "social partners" or Dutch "pillars," legislators, and decision-makers can in no way be replaced by the mere study of declarations of intent, administrative organization charts, or model equations. This motley set of factors provides a wide variety of resources, rooted in national networks with differing profiles. These profiles are not the result of broad cultural traits; rather, they are tied to birth processes, to specific and contingent selections whose outcomes consist of the State's practices, constructed by knitting knowledge and action together.

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EUROSTAT COLLOQUIUM

HISTORY OF BUSINESS CYCLE ANALYSIS

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BUSINESS CYCLES: REPRESENTATION AND MEASUREMENT

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The history of business cycle measurement is instructive in a number of respects, particularly in allowing us to understand various standard problems of economic measurement and their relevance for current concerns about business cycle indicators. From this history, we can make the distinctions between establishing the phenomenon of business cycles and establishing facts about business cycles, and between establishing measurements of the cycle and establishing the causes of the cycle. These projects were muddled in that early history, nevertheless, that conflation leads us directly into considerations about what is required for taking measurements of the cyclical activity of the economy. Questions about representation and the use of standardized quantitative techniques that arise from a study of the early 20th century work on business cycles remain informative for thinking about measurement problems in a similar domain of economics in the early 21st century.

1. Early Representations of the Business Cycle

A considerable number of graphic representations of the business cycle was produced by economists in the late 19th through to the mid 20th century. The favourite, and the most widely copied, kind was the Harvard A-B-C curves, originally constructed by Warren Persons (see Morgan 1990, chapter 2; Armatte, ibidem, figure 8). Several points about the nature of business cycles spring out from these representations, points which are well worth our notice.

First, as we learn from the visualization given in the A-B-C curve picture, this is a multi-dimensional concept: the business cycle cannot be represented by a single series of data but is represented by many series of different economic entities charted together.

Second, lots of information about the phenomenon of the business cycle are shown in the variations in the curves: their lack of regularity, their lack of congruence in timing, their jaggedness, etc.. Observed in this way, the business cycle is not a well-behaved, regular kind of phenomenon.

Third, although these graphs provide lots of information in the sense of data series and pictures, it is striking that it is not easy to pull out any measurements of the characteristics of business cycles (such as average durations, degree of variation etc) from these representations of them. At most, we could, like the early users, ask these indicators to provide dates of the turning points and the leads and lags between the series (though there are considerable difficulties in which curve should be used for that dating and how the timing of the curves should be related).

Fourth, the fact that this method of representation was immediately and widely adopted by many other business cycle institutes in the 1920s shows its extreme flexibility. The business cycles of different economies can be shown using the same basic ideas and methods of construction, but with curves particular to the national economy both in timing and in the series chosen. One good example of this is Lucien March's 1923 economic "barometer" (see Jovanovic and Le Gall, 2001). March produced a basic Harvard-style barometer (but with different elements), showing three different countries so that the charts could be used to point out the localised differences and symmetries between the cycles experienced in France, the UK and the US. In producing these pictorial, descriptive accounts of the cycle in the 1920s, economists conveyed their beliefs about the nature of the cycle. Thus, for example, in contrast to March's French conception, Ernst Wagemann's development of the methods in Germany took a somewhat different slant (see Tooze, ibidem).

Fifth, and to offer some modification of the third and fourth points, the example of March's work immediately shows us that, while not providing measurements of the characteristics of business cycles and whilst not being entirely standardized, the representations did not just provide indications of the direction that economic activity was heading. They also enabled comparisons within national boundaries (for example, to indicate a stronger or weaker than usual upturn) and for cross-national comparisons. The A-B-C curves were good at indicating things about the economic history and the current state of the cycle, but were, in fact singularly poor at providing measurements of the general characteristics of economic cycles. Like real barometers in relation to the phenomena of our weather, these business cycle barometers could be useful instruments providing information for every day usage but not for providing measurements of law-like behaviour.

The agendas and methods of different economists and statisticians moved then in various ways between description, forecasting and analysis (Desrosières, ibidem). From the point of view of measurement issues, the most important change was between the 1920s descriptive aims of the A-B-C curves and the analytical aim of representations we find in business cycle modelling work in the 1930s. One of the heroes of this move to analysis was Jan Tinbergen (see Desrosières, ibidem, and Boumans, ibidem). We can find a number of representations accompanying Tinbergen's famous Dutch model of the 1930s – famous for being the first macroeconometric model but equally important in the history of business cycle analysis. Tinbergen's representations in his later 1930s work on the US model were of several different forms: the set of data observations; the set of equations which made up his model; the individual relations showing how the various terms in each equation "stacked up" to form the individual measured relationships in the model; and finally a schematic arrow diagram showing the causal linkages between elements in the model at different time points (see Tinbergen 1939, and Morgan, 1990, chapter 4, for examples). Compared to the A-B-C curves, it is not nearly so obvious what these various different representations meant. Taken together they amount to all the elements that made up his statistical model of the underlying relations in the economy, relations which were both causal and dynamic for the model represented a dynamic process with a causal structure. This was not only the dynamics of historical events over time, but also the dynamics of a structure through time. For example, Tinbergen graphed each of the relations in his US investment analysis to give both the causal dynamic of the actual history of each particular variable (eg basic iron output) and to provide some sense of this part of the structure by "stacking up" (in a vertical display) the time paths of the causal variables in the relevant regression equation. Whereas the Harvard A-B-C curves attempt to reflect the path of the business cycle as indicated by the various data series mapped on the paper, Tinbergen's graphs aimed to analyse the causes of each main business cycle element and map the path of these causes and their outcome.

The other hero of cycle analysis was Ragnar Frisch (see Bjerkholt and Lie, ibidem). Frisch used little causal flow schemas as sketches for his business cycle theory in his 1933 "Propagations and Impulse" paper in which he worked out the economic structure of a business cycle model. These schema (reproduced in Boumans, 1999a) is of a very different format both from the A-B-C curves and the econometric models of Tinbergen. It doesn't give you any sense of the historical dynamics or timing but does suggest his idea of the causal relations involved. Boumans (1999a) has analysed the construction of this model in the context of his own account of economic modelling using his cake-making analogy of this morning. Boumans' account showed how Frisch's model was constructed by combining a rocking horse metaphor with the Slutsky effect: the structural modelling of the cumulative process analysis which, by the addition of a stochastic process of errors, can be simulated into the kind of dynamic data we find reflected in the A-B-C curves (see Morgan, 1990, chapter 3).

These two kinds of business cycle representations are of two very different things:- the former A-B-C curves are visualisations of cycle data into time-series graphics, the latter schemas and graphics of Tinbergen and Frisch are the representations of the business cycle into structural equation models. On the one hand, considering the barometers of the 1920s, we are dealing with the history of the attempts to represent the business cycle in a theoretically neutral mode, to picture the economy, to see how it moves cyclically through time. On the other hand, with the 1930s developments, we are talking about the history of attempts to build good models (theoretical and econometric) of the processes which cause business cycles.

I want to stress, and be more exact about, the points of that difference between the barometers and the structural models in relation to claims which might be made about the measurement of business cycles. The point here is not that historically we moved from a case of measurement of the cycle without theory in the 1920s to measurement of the cycle with theory in the 1930s. As already hinted in the above discussion, I have doubts about how far these early business cycle indicator projects should be understood as measurement projects at all, for, as noted, the power of the A-B-C representations as a device to measure the cycle was limited indeed. This (largely) 1920s project aimed to map the business cycle in terms of a set of indicators or barometer outputs. By contrast, in the 1930s work, we see attempts to establish the causal structures which result in business cycles. This latter set of business cycle models might be useful for suggesting economic policy towards cycles (as in Tinbergen's case), or in providing an indication of how to uncover periodic cycles hidden in the structural solutions (as in Frisch's case), but they too did not provide measurements of any general characteristics of any actual business cycle. While the move to the analysis of causal models may have been theoretically and policy progressive, it was not necessarily a progressive step in terms of measuring the business cycle. Thus, we can best interpret this interwar change as a move from representation of the cycle to representation of the causes, not from measurement of the cycle without theory to measurement of the cycle with theory. Measurement of the cycle was not the aim of either group, nevertheless, as we shall see, quantitative techniques to establish the data of the business cycle were an integral part of the indicator project.

2. Establishing the Business Cycle with Quantitative Rules

Perhaps the most important general question to ask of this early work on business cycle indicators, and a useful organising notion for later discussion, is: Were they trying to establish a phenomenon or facts about that phenomenon?

This is the question raised in a well-known paper in philosophy of science by Bogan and Woodward (1988) who wrote very carefully about the difference between establishing facts about the phenomenon versus establishing the phenomenon. Their example is the melting point of lead, a straightforward example: if you establish that the lead melts when you heat it, then you are establishing a phenomenon, but if you establish that lead melts at a certain temperature, you are establishing facts about the phenomenon. Logically, perhaps, you need to establish that lead melts before you establish facts about it. But, in the business cycle case, the problem rumbling around in this earlier period – and possibly still rumbling around – is that these activities were conflated. Nevertheless, analysis of the history and of the general problem of measuring business cycles relies on our clarifying the difference between establishing the phenomenon of the business cycle and establishing facts about that phenomenon.

Let me first suggest that, despite appearances, the Harvard A-B-C or indicator project is essentially about establishing the phenomenon, not about establishing facts about it – to use Bogen and Woodward's useful distinction. It does so by making the business cycle observable. And so the project is best interpreted in terms of techniques of observation, that is, the quantitative and statistical techniques produce data series which are then graphed so that the investigators can then observe, in the charts, the revealed business cycle phenomenon.

A comparison will help to emphasize this point. This indicator project of producing data in which cycles will be revealed to observation is clearly different from Wesley Clair Mitchell's quantitative or statistical project of "measuring business cycles" which was concerned with establishing facts about the business cycle. Mitchell's work provides the most strongly contrasting visual representations of business cycles available in the history of business cycle analysis. Mitchell and Burns constructed their "specific" and "reference" cycle diagrams in the 1940s at the National Bureau of Economic Research in the USA. These representations were designed to provide immediate measurements of many different characteristic aspects of the business cycle elements such as shape, duration, variability, etc. (see Burns and Mitchell, 1946 for the full report and Morgan 1990, chapter 2, for an example). Their calibrated diagrams (each had measurements attached) were the outcome of a long measurement project started by Mitchell before the First World War and only coming to full publication in 1946 at the end of the Second World War. To this day, these methods form the basis for dating US cycles (see Epstein, 1999). But more important for us, they provide an excellent foil to help us interpret that much more vital, and internationally wide-spread, indicator project which began in the early part of the 20th century and remains active today: most official statistical bureaux produce regular leading and lagging indicators of economic activity which are the direct descendants of the A-B-C curves.

This contrast in quantitative business cycle studies between the A-B-C indicator project and the Burns and Mitchell measurement project is comparable to the difference between establishing by observation that lead melts when you heat it and bringing measurement techniques to the question of establishing at what temperature lead melts. The difficulty in making the same distinction in this business cycle case perhaps lies in the fact that both of these processes, observation and measurement, rely on quantitative techniques and the manipulation of economic data[1]. Both of them look like measurement projects for both of them produce numbers. But the comparison clarifies that some techniques, the A-B-C ones, not only bring the phenomenon into observation, but by so doing they also go far to establish it as a phenomenon. The second ones, the NBER techniques, are used to establish facts about the observed phenomenon.

The second point to make in the context of this economics case is that these quantitative procedures are more than just tools of observation or measurement to establish the phenomenon and facts about it. The quantitative procedures of the indicator project used to observe/reveal the cycle in economic activity at the same time defined the phenomenon of the cycle. It is in this sense that we might almost say that the quantitative procedures "created" the phenomenon; at least in establishing its details. Such a role for quantification is well rehearsed in the work of the renowned historian of the social sciences, T.M. Porter, who, in a 1994 essay, wrote that "standardised quantitative rules have been almost as fertile as standard experiments and mass produced instruments in the making of new things" (Porter, 1994/6, p 36). In other words, in the social sciences, we come to an understanding and appreciation of new phenomena not by a series of experiments, nor by the development and adoption of physical instruments, but by establishing quantitative techniques to access them and, over time, these techniques then become standardized and our knowledge of the phenomenon becomes likewise fixed.

The phenomenon of the business cycle was in some sense a new one in the early 20th century. In the early 19th century, most economists thought in terms of irregular crises manifest in harvests and grain prices; later this turned into an irregular, but limited, external trade cycle. The notion of a rather more regular and more generally widespread business cycle really took off in the late 19th and early 20th centuries. So, it is important to keep clear in our minds that economists and statisticians were, in this early period, essentially grappling with both the problem of establishing that there was such a thing as the business cycle at the same time as defining its nature and qualities and attempting to determine some facts about it.

[1] A similar dual process might be observed in a parallel economic and statistical case of the period, namely in work on the distribution of income. Using statistical and quantitative techniques and a graphic representation, Pareto and Lorenz made the distribution of income (the phenomenon) observable; the measurement of income inequality (establishing facts about the phenomenon) was provided by the Gini coefficient which was easily interpretable in terms of the observed Lorenz curve. I owe this example and thanks to Pedro Teixeira (2002), whose paper helped me clarify this point.

We can follow these conjoint problems through the literature (see for example Michel Armatte's paper in this book which shows a wonderful time trawl through attempts to represent the business cycle). In that early period there was no collective agreement on what the business cycle phenomenon was. The questions for those earlier years (and they may still be relevant) were: Is the business cycle a cycle in output or prices or something else? And what is it like: Is it regular or irregular? Is it multidimensional or unidimensional? Are cycles homogeneous or are they always particular and different? Are all countries' cycles the same? These are the questions which are in a sense going round and round internationally between the various research institutes and national statistical agencies of those days. Most of these questions are questions of fact about business cycles but they are also questions directed at clarifying and conceptualising the phenomenon of the business cycle itself. It is problematic to conceptualise a phenomenon and clarify its characteristics at the same time as trying to establish numerical facts about it. There should be no surprise then that, historically, it proved extremely difficult to get conceptual agreement on the business cycle (either as a statistical phenomenon or in economic theoretical terms). It was difficult to establish facts about the cycle because it was very difficult to establish the phenomenon in the first place.

Amongst all these difficulties, the Harvard A-B-C techniques (in their many national versions) were critically important in establishing the, then relatively new, phenomenon called the business cycle and in effect their techniques defined the cycle. What were these techniques? The Harvard A-B-C method uses a basic decomposition method: you begin with a time series of economic data, you de-trend it and then you smooth it to get rid of the small changes and, lo and behold, what you have left is a data series which is combined with other similarly treated series to create the charts of the business cycle. Thus the cycle, and so to a large extent its measurable characteristics, are defined by the initial quantitative procedures[2]. The project of establishing the phenomenon effectively crosses into that of defining the cycle and of providing measurements or data for it. Establishing the data in which the phenomenon would be revealed crosses into providing measurements of the business cycle itself. Of course this all creates a difficulty of circularity. Can we break into this impasse and, if not solve the definitional and the measurement problems here, at least see what is needed to do so?

In this context, we should note two important points from Bogen and Woodward's lead example. First, you do not necessarily have to have a theory about what causes the phenomenon of lead melting, but you do need to have the concept of lead melting in order to establish facts about it. If you have a well defined concept, you do not necessarily need to have a good theory of causes and effects about it before you can establish facts about it. This runs contrary to claims that appeared in several of this morning's papers: the traditional view of the role of theory, namely that you cannot have measurement without theory, that theory is necessarily prior to measurement, and that the term "theory" here means theory about how the economy works, i.e. economic theory. I suggest that for establishing the phenomenon and facts about the business cycle (both the raw A-B-C data and any derived summary statistics), that is, measurement, as opposed to explaining the causes of business cycles (or business cycle analysis), the need to have an economic theory of the cycle is over-rated. You might think this claim about the non-necessity of theory is a somewhat bold claim that I am not willing to stand by, but let me point once again to support from Porter (1994), who reminds us that the history of science is ridden with examples where there is theoretical disagreement over the nature of

[2] On the question of circularity, and why it may not be critical, see Chang (1995). There is strong criticism on these grounds in a related case in the 1930s by Abraham Wald who discussed the impossibility of gaining a reliably measured concept where the quantitative or statistical procedure both defines and establishes the data series for the phenomenon at the same time (see Hendry and Morgan, 1995, Reading II:12). Note also, that the business cycle data set, constructed in this way is not a positively defined data set, but a residual one, for it is defined as what is left over when you have taken out the other bits. Thus, there are in fact two problems: first that the data series in which we see the cycles, are residuals and, second, that the process of getting the residual data set is also responsible for defining the phenomenon!

things or there is a lack of knowledge of the causes of something, but nevertheless that did not impede measurement. You can measure things without necessarily making a commitment to a cause or you can make a measuring instrument on the basis of a theory which afterwards proves to be completely wrong. Indeed, a good example is given by Chang (2001) who tells us, in his history of the thermometer, that the theory of heat of the time was soon proved completely incorrect. However, while a correct causal account for the phenomenon may not be necessary for measurement, you do need conceptual resources of some sort, and these may well come from economic theory.

Secondly, something else is needed to establish the melting point of lead, namely, a reliable measuring instrument and techniques for its use. Our parallel example implies that we can expect to measure business cycles only if we have a good measuring instrument available to do so, and this brings us back to our problem with business cycles - that early investigators were trying to define the cycle and produce techniques for measurement at the same time. Let me take up the importance of measuring instruments in economics first and then return to a discussion of the role of concepts later.

3. Strategies for Making Measuring Instruments

It seems to me helpful to consider questions of economic measurement explicitly in terms of what measuring instruments we have available in economics, and how they are constructed (see Morgan 2001)[3]. We can understand how problems of economic measurement are resolved at three levels. We can think of there being <u>measurement strategies</u>: strategies for making <u>measuring instruments</u>, and then using the measuring instruments to take <u>measurements</u>. For example, there is a general strategy for making thermometers, and any particular type of thermometer is the measuring instrument with which we take measurements of the temperature of specific things. Historically in economics, it is not the case that economists thought of the strategies and then made the instruments. No, chronologically, the first problem was working out how to make a measuring instrument to take measurements of a particular economic entity, and then, and only with hindsight, did economists start seeing that sometimes such an instrument was one of a more general kind, that they had a measurement strategy which could be applied to lots of different things.

This may sound very general, so let me start by filling in what I mean by "measurement strategy". One example is the strategy of weighted averages. It is a wonderful strategy which allows you to make measuring instruments for all sorts of different things in economics, namely, all those cases in which we need to add together things which are unlike. The principle involved in this strategy, and for all measuring instruments of this kind, is one of adding elements, weighted according to some share in the whole, into an aggregate to represent the whole. This basic index number strategy is something which evolved through the work of a number of individuals like Paasche, Laspeyres, Fisher and Mitchell to become a generally useful strategy for creating a family of related measuring instruments for economics.

Another useful example is found in the strategy of accounting. This strategy makes use of accounting rules and procedures such as balancing requirements, identities, and the condition that things have to add up to 100 percent. These are very useful principles which have been used in very different kinds of measuring instruments, in economics. We can think of both the national income accounts and input-output accounts as being two very different measuring instruments constructed under that same accounting strategy. Both sets of procedures depend on accounting kinds of principles, but they measure different things and the standardized quantitative rules they follow are also different.

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3 I have provided a more general treatment of the measuring instruments of economics as a result of thinking about the papers in an edited volume on the history of economic measurement (see Klein and Morgan, 2001). This commentary draws on that experience and on the papers on models in relation to measurement reported in Morgan and Morrison, 1999.

Each of these different strategies (such as the weighted average strategy, the accounting strategy) involve matters of principles: rules or constraints about how you construct the measuring instrument, and those typically go along with some kind of criteria. That is, accounting rules or principles provide you with criteria on whether or not you have made an adequate measurement: "Do they add up?" "Have you covered everything?" "Have you counted things twice?" So the rules and criteria tend to go along side each other in the sense that the rules come with criteria attached so you know you have met them. Other measurement strategies for example might involve principles from statistics and these are accompanied by the whole panoply of statistical criteria.

It is the principles involved in these strategies which allow you to get some kind of representative power in measurements[4]. Let me express this point somewhat differently. The principles are really important because it is these which hold the elements of these measuring instruments together. They both provide the rules and constraints of construction and are associated with the criteria to validate their workings. This enables us to feel confidence that our measurement devices really are measuring the thing we want to measure in the world. This is naturally of some importance for we want confidence that the things we are measuring are actually the kinds of things we have in the economy.

Again, an example may helps us here. Let us suppose we use the weighted average strategy to construct a price index formula to measure the general level of prices. How we are going to make sure we have really got all the measurement rules working in our measuring instrument and that we can cover all the things we need so that when we use it, we gain some kind of representative power over the things we are trying to measure? More prosaically: How we are going to make sure we have got all the prices in? Have we got them weighted according to the right amounts?[5] And have we chosen the correct index formula for the question in the first place? All these kinds of questions are dealt with by following the principles of index number construction and by taking account of the criteria that come with these principles. For instance, index numbers are required to satisfy certain theoretical consistency criteria and at the same time some empirical criteria. (see Boumans 2001). Those two elements, principles and constraints, and their associated criteria, are what gives us confidence in our measuring instruments.

Specific measuring instruments in economics are made according to a more generic measurement strategy, and involve principles and criteria. But the successful construction and application of any measuring instrument in economics involves both a set of quantitative techniques and elements of judgement or skill. For example, take the case of index numbers again: the raw data on prices and quantities have to be collected, cleaned of error, translated into usable form, assembled and aggregated according to a formula and then calculated to make the final index number. This whole process of applying the measuring instrument involves both numerous techniques and skilled judgement at many points. But, in the absence of principles or rules, techniques and skills do not themselves provide the representative power and criteria we need for a good measuring instrument, as we shall see in the business cycle indicator case.

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4 Indeed the basic widely accepted philosophical theory about scientific measurement rests on "representational theory" (see a useful survey with references to the fundamental work, see Finkelstein, 1982).

5 This is perhaps rather like the syntax and semantics that Michel Armatte mentioning in his paper (see ibidem). We might think of these principles for making measuring instruments and the criteria associated with them as interpretable in terms of his syntax and semantics.

4. The Problem of Interpreting Business Cycle Indicators as Measuring Instruments

The business cycle indicator procedures cannot be considered measurement instruments. The A-B-C curve techniques were measuring devices of a kind, but they did not prove good for providing measurements of the business cycle because, I suggest, they do not have principles which hold the elements of a measuring instrument together and which provide the constraints and the criteria which would link their measurements to the world.

The data produced from the early business cycle indicator project come from measuring techniques which are generated by a reduced measurement strategy not from a full strategy. As suggested above, and discussed more fully in Morgan (2001), measurement strategies consist of three elements: principles, judgement and techniques. These early business cycle indicators of the A-B-C type are generated by a measurement strategy which is dominated by judgement and techniques but in which principles are missing. As we have seen, providing such data on the business cycle consists of collecting several sets of time-series observations, de-trending the series, smoothing them, and combining them according to some further techniques, to create a business cycle indicator. But this constitutes a set of techniques – not principles – and those techniques are not generated by rules which have associated criteria which enable us to be confident we have a good measuring instrument.

At this point, it seems legitimate to ask: What might a rule or principle look like in this case? Let me take as a comparisons, the two simpler, but contrasting, cases of measuring which are by-products of this indicator work, namely, the measurement of seasonal variations and trends. Seasonal variations have a period fixed by nature, a regular exogenous event outside the control of economic activity which fixes the period for measurement - it is a year long period of variation, fixed you might say by the concept itself. (A similar man-made period might be enforced by institutional rules such as taxation demands.) The concept thus provides an immediate rule, that statistical investigations must be focussed on variations which repeat themselves within matching 12 monthly (say) periods. We can use some kind of an averaging criteria over the period, partition out the additional variation, and by this rule measure the seasonal element. The concept connects closely with a principle and criteria for realising this in a "standardized quantitative rule" for measuring the seasonal variation in a series. Contrast this with the problem of measuring a trend, also investigated by those economists measuring business cycles and seasonal variations. We have a variety of techniques for measuring trends, but any measurements found depend critically on the period chosen for the measurement. Although there are some simple rules here for choosing periods (e.g. peak to peak, not peak to trough), there is no natural period or concept defining a trend variation, unlike the case of a seasonal variation. The choice depends on judgement. Economic historians will tend to take war years as stopping years. Statisticians have developed packages for measuring trends and seeking out changes in trends, using statistical criteria. But these statistically defined measurements may have no obvious hooks onto any economic concept of a trend.

The case of business cycles is more like that of the trend measurement than the seasonal variation measurement. There is no concept of a "natural period" for business cycles, so the seasonal adjustment route to principles is not available. If we had a widely accepted economic concept of the business cycle as a regular periodic cycle, as Jevons or Moore had in those early years (see Morgan, 1990, chapter 1), we could, like them, use this to define the cycle and provide a principle for measuring both the periodicity, and certain other characteristics of it, all with associated criteria. But modern economists don't really believe in periodic cycles – it goes against our experience of the business cycle, just as it went against the beliefs of most other economists working in the early 20th century. This lack of any concept-related principle is one reason why these

A-B-C indicator measurements were so flexible to varying national experiences, but that same lack of principle is also indicative of the lack of constraints or rules and criteria which may continue to bedevil business cycle measurement. Business cycle measurement in the early 20th century remained in the same condition as trend measurement - business cycles were determined by judgement on an individual case and by a series of quantitative techniques hardly related to economic concepts.

There are however other sources of principles for measuring instruments besides exogenously or institutionally fixed time periods of economic activity. This is where we return to the role of concepts. Recall my claim earlier that we don't necessarily need the causal theories of the cycle but we do need economic concepts of the cycle. The reasons we need concepts is that they provide a rich source of possible principles for measurement instruments. Where do we find such concepts? One place is in analogies and we have two very good examples in this book this morning[6]. Michel Armatte's paper presents some medical analogies of seeing the economy in terms of a healthy or unhealthy body and its symptoms, symptoms being the things with which we could track the health of the economy. There is a long tradition of understanding the economy in terms of bodily health, which provides us with quite a lot of concepts, and these gave some grip into the measurement structure for those who wanted to provide data for the business cycle. Marcel Boumans (see ibidem) presents some physical analogies and again thinking of the economy as a mechanism is a long standing analogy which has provided us with concepts which relate to various measurements structures in economics including those for business cycle analysis.

But analogies and non-analogical economic theories theory rarely tell us directly how to take measurements. Nor do they usually give us enough resources to make measuring instruments. A parallel example may help again. Theories of heat do not tell you how to make a thermometer. The concepts of such theories may be suggestive about principles, but you also need to know an awful lot of things about the behaviour of various materials such as, for example, glass, alcohol and mercury. In addition, the principles require that you think about what you want your thermometer to do, namely to be reliable under various different circumstances of time and place and that in turns brings you to the development of measurement principles and criteria some of which may well be far removed from the theory of heat (see Chang, 2001).

We had a good example of the concepts in economic theory providing possible principles for measurement instruments when Jacky Fayolle (see ibidem) talks about Wicksell and his cumulative process analysis. Wicksell's cumulative process idea is a good conceptual tool that relates to his theory and has provided resources for business cycle model building. It is well known that one can understand the propagation part of Frisch's structural model in Wicksellian terms, and one can also understand Tinbergen's model as a cumulative process model from looking at the mathematical moulding that Marcel Boumans was referring to. Clearly there are theoretical resources in Wicksell's idea which can be used to provide concepts for business cycle causal analysis, but these are not necessarily going to be directly the ones we need for measurement of the cycle itself. Neither Wicksell's theory nor the concept tell us how to go out and make measurements which will be relevant for business cycle study. If we were to use Wicksell's concepts and theory, we might find a principle for a measuring instrument for the business cycle by looking at the difference between the bank rate of interest and the real rate of return on capital. This sounds straightforward, but to provide the

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6 At this point, the reader may suggest, that once you use analogies, you are using theories, and so, of course, theory does matter after all. Well, yes, it can matter, but we have to look more carefully at how and where it matters. Traditional philosophy of science pictures a hierarchy with theory at the top and data at the bottom. Theory may be at the top for theory justification purposes (though I think in Marcel Boumans' diagram of model construction - see ibidem - it was somewhere to the top-left), but this does not mean it has priority for thinking about measurement problems. So the basic message that I propose here is that we need to rethink exactly where theory comes in, and exactly how far theory was helpful in the measurement strategies that we saw in both the business cycle indicators and business cycle models.

techniques to measure the latter notion immediately gets us into huge conceptual difficulties and aggregation problems, and it is unclear how this instrument provides a measuring tool to get at the multi-dimensionality of the cycle (as seen in the A-B-C curves).

In practice, Wicksell's theory does not even provide enough to create a model to represent the cycle in ways which relate to observations. Thus if we look at Frisch's work, we saw that he had to add on another part - the stochastic or Slutsky effect. If we look at Tinbergen's work, we see that he had to add a whole lot of variables and define their time relations to get models which could be used in a measurement project. Marcel Boumans (1999b), has pointed to the fact that whereas theories may not give you the measurement instructions directly, it is sometimes the case that, when you move from a theory to a model which incorporates the theory, there may be part of the model which links on to a measurement process. So models may include particular instructions or formulae for making a measurement instrument, even when the theories which they embed do not.

Another example shows where economic theory has been more helpful in providing concepts. Keynes' theory of the aggregate economy does provide the conceptual resources for thinking about measuring GNP. This is one of the few cases where not only does theory provide conceptual resources but it provides them directly in the form of the basic categories and in the principles (the accounting principle) that you need. So there is a rather closer link between theory and measuring principles in this case than is usual in economics and perhaps this is why this is the standard example that is given in economics for a strong theory-measurement link. The Keynesian concepts of deficient and excess demand might provide a principle for a measuring instrument for the business cycle, but once again it lacks the possibility to access the multi-dimensionality associated with the business cycle.

Though there may be many conceptual resources available within economic theory for thinking about measuring business cycles, choosing one which provides suitable principles for a measuring instrument for the cycle may be a very difficult task. As in the case of thermometers, the principle has to be adequate to certain characteristics of the phenomena but must also be workable given the technical applications in mind.

5. Establishing New Things with "Standardized Quantitative Rules"

Let me return now to Porter's views about the power of quantification in the social sciences. Porter's "standardized quantitative rules", or, in my terms, "measuring instruments", contribute to the making of new things in the world, in the sense that if we start to make good measurements of things then we may well come to believe in those things. If we construct a Euro-zone business cycle indicator we will have a Euro-zone business cycle. Of course this perception of the "reality" of such new things does not come immediately. But once you have created a standard measurement, and used it to such an extent that it comes to fit onto the world, then you have created what some people call new "ontic furniture". In other words, those elements which have first been "created" via a quantification regime become things which we take for granted in the world.

Adam Smith wrote about the wealth of nations in 1776 and conceptualized it as some kind of economic richness in the national economy, but it was only after 1870 that we get real attempts to measure something like national wealth, only in the 1930s that we get concepts of something like GNP (or GDP), and only in the 1940s that the concept was associated with a good measuring instrument to establish it as "fact". Thereafter in the 1950s, GNP moved from a category of analysis for economists in government to a standardized international measurement (to be measured according to a set of Porter's "standardized quantitative rules" and associated criteria, see Comim, 2001) and soon became an everyday number which governments use and businessmen expect to find printed in the national newspapers: literally we find ourselves accepting a new economic entity in the world.

We might also note here that the concept of measured GNP started off being tied to some particular macro-economic notions, but has since cut loose from those theoretical backgrounds and has become a theory neutral if not a value neutral measurement (an important distinction) of the economy's progress and health. It is also worth reminding ourselves that just as there is no one theory bound to this concept, there is no one right set of rules for constructing GNP measurements which fit automatically and absolutely onto each country for all time. Nevertheless, all GNP figures are constructed according to a set of internationally established principles and criteria which come from its basic accounting framework. Thus GNP figures are constructed according to principles and criteria for measurement, and the application of this measuring instrument to individual cases and countries, and for particular purposes, relies on a set of techniques applied with judgement.

The GNP measurement instruments evolved over a period of time and the measures they have made come to seem to fit on to the world even though they do not of course fit very well to the world (as, I think, someone reminded us – in the discussion) The measured GNP has come to seem a natural kind of thing in the economy. My analogy here is: if you have to do your own tax accounts you are presented with a form from the tax office which essentially says: "divide your economic activity into these categories". Your own activities never quite fit the taxman's categories, but you learn how to make your life fit into his categories so you can report yourself in the right way and then you begin to think in those categories when you are keeping your accounts and doing your filing system.

A similar example can be found in the CPI, the consumer price index. This is a measurement which people regularly look at, regularly use in wage negotiations, regularly use as an instrument for policy action, and take for granted as much as the daily temperature. We probably do not consult the CPI figure everyday, but whereas we might need to know the daily temperature before we set out in the morning for work, there are lots of times when people will want to know what the CPI change is in order to make all sorts of decisions at lots of different levels. But of course the CPI, like the GNP, did not exist a hundred years ago, though now we treat them both as real things in our world. Those two new things are the result of the development of measurement strategies by economists and statisticians over quite a long period and the struggles they had to make those pieces of economic furniture are long since forgotten.

The now standardized quantitative rules and procedures are measuring instruments with which we construct the GNP figures or the CPI figures. They are the thermometers of economics: we use them to measure the GNP and to measure the CPI and tell us what is happening to our economies. Like thermometers, they provide the popular indicators of what is going on in our economic life. We become so used to them that we come to take them for granted. We do not worry about what causes the mercury in our thermometer to rise, all we need to know is how to use the instrument. Similarly there is a commitment in the GNP or CPI user group not to worry about what causes any changes, and economists can hold totally different theories about this, and yet they will all use the same set of measurements, coming from the same measuring instruments.

If you think about Porter's claims in the context of our discussions about business cycles, you can realize how really surprising the situation is. People have believed in the reality of something called "business cycles" for about 100 years. Everybody in the streets of the Euro-zone believes in business cycles: it is a common property concept but it is not one attached to any good common property measurement unlike those for the CPI and the GNP, despite the fact that the common usage of the concept of the GNP is very much younger. This lack of accepted common property measurement may have little to do with whether the Euro-zone has one converged cycle or still a series of national cycles. It may rather be related to the fact that regardless of facts about the European economic and monetary union, there is no agreed principled measurement of a cycle which has become generally accepted and widely used; there is no accepted measuring instrument or standardized quantitative rule. The challenge for Eurostat is to make a business cycle measurement instrument which generates measurement of the business cycle that gain the same kind of economy wide status as a CPI or GNP measure, and thus places numbers on our already well-accepted economic furniture. There are however dangers to be avoided.

6. The Dangers of Standardized Quantitative Rules and Unprincipled Measuring Instruments

We can use the history of this earlier period, and the analysis of today, to suggest two possible dangers that stem from standardization and the lack of principles and associated lack of constraints, rules and criteria, in measuring business cycles.

Representation without standardization constitutes the first danger for a business cycle measurement project. Business cycle indicators of the early period did provide some kind of local representative power, that is they did fit the economy of the time and place because that is exactly what they were designed to do. But the techniques those economists developed did not constitute a measuring instrument which had a strong enough representative power to travel. Their techniques did not necessarily travel over time because the economy changes and comparisons were compromised. The techniques did not necessarily travel over space since each national institute founded their own indicators and this meant a lack of standardization and so lack of comparability.

The local nature of these representations had further implications. It seems to me that those economists who spent so much time working on those wonderful business cycle indicator charts were looking for a bigger payoff than they gained[7]. They believed that their complicated A-B-C curves would provide the kind of access which would allow them to understand and explain the business cycle and to analyse its causes. That is, economists hoped the A-B-C curves could be used not just for information but to provide some analytical grip on the economy. I think on the whole this promise was not fulfilled due to the lack of principles, the lack of real concepts, involved in their construction and the associated extreme localness of their representative power.

Standardisation without representation is the second danger. We can easily imagine standardising business cycle measurements across national borders using some basic widely accepted standardized quantitative techniques (and perhaps this has already occurred[8]) but if those techniques were not based on any principles of construction and lacked associated criteria, we would lack confidence that they represent what has happened in the economy.

This goes back to my earlier point about business cycle measurement not being linked to a clear concept. We expect to find the principles of the measuring strategy linked to a concept, so in business cycle measurement, we are, in some sense, lacking all the way down the line a very well defined concept that gives us these principles. For example, in macroeconomic measurement, the concept which underlies the aggregate economy measures of national income accounts is that of an economy as an household or business accounting unit. This concept leads us to accounting principles which gives us the criteria whether or not we have made a good measurement of GNP, though "good" here of course is subject to lots of different interpretations. In business cycles on the other hand, we lack a real principle, a real concept either from theory or by analogy. We might standardise across the Euro-zone with techniques, but if we want our Euro-zone indicators to do more

7 One reason that Frisch and Tinbergen moved to models of the business cycle is because they were looking for something which was not just a local measurement structure, but also provide the information for an instrument of analysis.

8 Information available at the Eurostat history meeting from the project "BUSY" suggests that this standardization has not yet occurred, although the objective of the project refers to the development of tools for a "standard business cycle analysis".

than create a temporary Euro-zone economic psyche, if we want them actually to deliver us some measurements which matches the economic experience of Euro-zone people (for example when they are unemployed or employed, or are subject to big price inflation or even deflation) as the cycle moves on, we need to have measures which match experience, that is, which provide representative power.

This representative power matters precisely because of the point that Porter makes – we come to believe these measurements are indicative of something real in the economy. And, if our economic experience is out of joint with the measurements, we are more likely to distrust the measurements than the experience. There are many examples of governments rejecting official or standardized measures of things on the grounds that they no longer measure what is going on in the economy. This maybe because the numbers are politically difficult (which itself speaks to the power of those standardized numbers), but it maybe because standard rules sometimes get out of joint with the changes in the economy (as in recent discussions about how the "new economy" is reflected in macro-measurements). We equally have examples of other economic groups reacting in the same way when their experiences leads them to distrust the numbers – for example, during WWII in the USA, unions rejected the CPI figures as accurate measures of the changes in the cost of living (see Banzhaf, 2001).

Business cycle measurements need to have representative power to be taken seriously and simple standardization of quantitative techniques may well be insufficient. If Porter's "standardized quantitative rules" are interpreted as measuring instruments: techniques built on concepts and principles with associated criteria, they offer a better chance of gaining acceptance and success in providing meaningful measurements. Standardization without representation is therefore a potential problem with wider impact than representation without standardization.

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MACROECONOMICS DENIED:
GERMAN BUSINESS-CYCLE RESEARCH 1925-1945

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TABLE OF CONTENTS

The fashion for business-cycle indicators, which swept the world in the 1920s, left a particularly important legacy in Germany, in the form of the Berlin Institut for Business-Cycle Research (Institut fuer Konjunkturforschung, IfK)[1]. The Institute was founded in 1925, by the entrepreneurial head of Reich's Statistical Office (Statistisches Reichsamt, SRA), Ernst Wagemann, to complement official data gathering with adventurous economic analysis[2]. By the 1930s, the Institute had grown into the largest and the most prominent institution of economic research in continental Europe. In 1933, during the Nazi seizure of power, Wagemann was forced to resign as President of the Statistical Office and to concentrate his attentions on the Institute. This has sometimes given rise to the impression that Konjunkturforschung fell under a political cloud. Added to this the intellectual programme of business-cycle research was buffeted in Germany, as elsewhere, by the experience of the Great Depression. In fact, Wagemann's removal from the Statistical Office was the work not of the Nazis, but of Hitler's coalition partners in 1933, Hugenberg's German Nationalists. Once the National Socialists had consolidated their monopoly of power, Wagemann and his Institute became favoured collaborators of the regime. Konjunkturforschung also survived the 1930s as an intellectual project. In fact, during the war it was to experience its moment of greatest influence. In the wake of Hitler's armies, the IfK took charge of the existing research establishments in Vienna, Paris, Prague and Amsterdam. Renamed in the triumphant summer of 1941 as the Deutsches Institut fuer Wirtschaftsforschung, the business-cycle research Institute of the Weimar Republic became the nerve centre of the improvised system of central planning through which Albert Speer struggled to save the Third Reich. There was a real intellectual continuity, which connected the models of central planning experimented with during the war, to the business-cycle research of the 1920s. Indeed, the legacy of the 1920s survived even 1945. The descendants of Weimar's Institut fuer Konjunkturforschung, the DIW in Berlin, the Rheinisch-Westfaelisches Institut fuer Wirtschaftsforschung in Essen and the ifo in Munich have continued to play a key role in German economic policy discussion. Alumni of the Institute have occupied a variety of prominent positions in both German and European public affairs. Perhaps most notable in the present context, is Rolf Wagenfuehr, who serving as chief statistician to Albert Speer's Ministry, became in the 1950s the founding head of the statistical division of the European High Authority and, ultimately, Eurostat.

In his paper in this volume, Alain Desrosieres has proposed an admirably capacious framework for a comparative history of modern macroeconomic modelling. Let us start by situating the history of the IfK and the associated work of the Statistisches Reichsamt in his terms. According to Desrosieres, the emergence of the modern complex of macroeconomic modelling was a multi-faceted process, embracing the development of no less than eight different aspects of economic research and analysis. For sake of brevity I shall label these Desorieres Approaches 1-8 (DA 1.-8.). Not surprisingly, the IfK in its work before 1945 did not achieve a complete synthesis of all eight elements. Even the precocious Dutch did not come close until the 1950s. By comparison with Tinbergen and his team, or the Norwegians under Frisch, the chief deficit of Wagemann's establishment was its lack of mathematical sophistication. The IfK did sponsor and publish one of Europe's first exercises in modern econometrics in the 1920s (DA 6). But, this remained a one-off. In technical terms, Wagemann's Institute was limited to conventional exercises in index number construction and analysis. Not surprisingly, therefore, a properly probabilistic understanding of economic models (DA 7) was also out of reach for German economic research until after World War II. But, the range of the IfK's activities and the degree of institutionalization, which Wagemann was able to achieve, were remarkable, especially when one considers the turbulence of German politics in the interwar period.

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[1] R. Krengel, *Das Deutsche Institut fuer Wirtschaftsforschung (IfK) 1925 bis 1979* (Berlin, 1985).

[2] A. Wissler, Ernst Wagemann: Begruender der empirischen Konjunkturforschung in Deutschland (Berlin, 1954).

The economic research of the Weimar Republic could, of course, draw on a long German tradition of quantification and large scale statistical enquiry inherited from the 19th century (DA 1). Around the turn of the century, German University economists also became increasingly interested in holistic analytical models of the economy. Unlike in France, this did not take the form of a preoccupation with general equilibrium analysis. Instead, it was early monetary macroeconomics and Marxian reproduction schemes, which provided the basis for an understanding of the economy as a systematic and integrated system (DA 2). Germany and Austria also shared in the general upsurge in interest in cyclical phenomena in the late 19th century (DA 3). In fact, in an age before English became the dominant academic medium, German was one of the main languages for the Europe-wide debate about the business-cycle. Until World War I, however, this intellectual exchange was confined to the academic sphere. The apparatus of official statistics in Imperial Germany remained limited to the social and demographic repertoire defined in the 19th century. The Kaiserliches Statistisches Amt produced hardly any economic statistics in the modern sense of the word.

It was World War I that shook up the German state apparatus and brought into government a new generation of University trained economists. Amongst these was Ernst Wagemann, a specialist in colonial economics with a monetary flavour. During the 1920s, Wagemann exploited the state-building tendencies of the Weimar Republic to create in the Reich's Statistical Office and the IfK a new national hub for both economic data-gathering and economic research (DA 8)[3]. Wagemann's ambition was to install empirical economic research at the heart of the newly emerging apparatus of national economic policy. The hopes of achieving this during the Weimar Republic were dashed by the unanticipated shock of the Great Depression. However, there can be no doubt that the unprecedented output of statistics and quasi-official economic commentary by the Statistical Office and the Institute were the main forces in transforming public debate about the economy. And, during World War II, the Institute was to gain a direct channel to the highest level of government. However, only at the expense of allying itself with Hans Kehrl, Speer's chief of planning and one of the most radical Nazi technocrats.

Funded generously by the German state, business associations and the labour movement, the Berlin Institute was the best-equipped laboratory of empirical economics Europe. Wagemann's staff made a heavy investment in the techniques of time series analysis, which were the common stuff of barometer research in the 1920s (DA 4). In the form of Hanau's work on the hog-cycle, the Institute also sponsored and publicized one of Europe's first exercises in econometrics (DA 6). However, the title of Wagemann's Institute for Business-Cycle Research was in many ways misleading. The dominant preoccupation of Wagemann's research establishment was neither Harvard-style cyclical analysis, nor econometrics. What gave coherence to the Institute's work, was the effort to construct a fully comprehensive system of national accounts, including estimates for all three components of the economic circular flow or 'Kreislauf': income, production and expenditure (DA 5 a., b. and c.). The trinity of estimates for income, expenditure and production was completed in 1936, with Germany's first census of industrial production. This, in turn, provided Rolf Wagenfuehr and the Institute with the statistical tools necessary to bring some semblance of order to the wartime economic planning of the Third Reich.

The IfK-SRA nexus thus embodied at least six, arguably seven of Desrosieres eight facets of modern macroeconomic analysis. It is, therefore, a highly complex object. As Desrosieres has pointed out, this multi-facetedness means that modern economic knowledge has multiple histories, each emphasizing one or more of the eight aspects. To make my subject manageable, I will focus in this article on just a few elements: the relationship between the cyclical analysis of the Institut fuer Konjunkturforschung, the barometer system it devised, and the underlying macroeconomic conception that informed its work.

[3] J.A. Tooze, *Statistics and the German State, 1900-1945. The Making of Modern Economic Knowledge* (Cambridge, 2001).

1. The Development of the IfK's Barometer System

The IfK and its barometer system were born directly out of the crisis of the German state in the wake of World War I. In the midst of revolutionary turmoil in the winter of 1918-1919, the first President of the German Republic, Friedrich Ebert, ordered a monthly report on the state of the national economy to be submitted to him by the Reich's Ministry of Economic Affairs (RWM)[4]. Responsibility for these reports fell to the statistical section of the RWM's economic policy department, which was headed after 1920 by Ernst Wagemann. The reports themselves seem to have been written by Adolph Loewe, who was later to make a name for himself as a University economist. Initially, the reports made an unpromising start. The early reports of the RWM consisted of a summary of administrative actions taken by various branches of the Ministry. Only in 1920, did they begin to develop into an overview of the state of German industry and commerce. Initially, this amounted to little more than an unstructured list of unanalysed data. It was not until 1921 that the reports began to generalise about economic conditions and to employ the buzz-word, Konjunktur (business-cycle), to summarize the upward or downward movement of the economy[5]. In 1924, Wagemann was appointed as the new President of the Reich's Statistical Office. Adolph Loewe moved with him to take charge of a new section for business-cycle observation[6]. The result was a dramatic intensification in reporting. In the autumn of 1924, the Statistical Office published its first business-cycle indicators, styled after the barometer system of the Harvard Committee of Economic Research[7]. The figures were published in the form of a weekly bulletin entitled *Deutsche Wirtschaftszahlen*. And they certainly seem to have found an appreciative audience. The Reichschancellory ordered three copies for its internal use.

In Germany, as elsewhere in Europe, the Harvard barometer was the most well-known system of short-run economic prediction in the early 1920s, frequently reproduced in the business press. It owed its popularity, at least in part, to its endorsement by John Maynard Keynes[8]. At the time, Keynes was convinced that the barometers provided a valuable tool for stock market speculation. Unselfishly, he published a description of the Harvard Committee's techniques in the *Reconstruction Supplement of the Manchester Guardian*. In the straightened circumstances of the postwar period, when library budgets were under severe pressure, this was one of the few foreign publications that was widely accessible in the German-speaking world. From the start, however, Wagemann and his staff did not apply the Harvard barometer uncritically to German circumstances. Whilst appreciating the American statistical techniques, Wagemann and Löwe were suspicious of the Committee's simplistic empiricist methodology[9]. Pearson the chair of the Harvard Committee claimed that the statistical series included in the Harvard barometer were selected without regard to any preconceived theory. However, to German eyes the system appeared to be based on rather parochial assumptions. In particular, the Harvard system relied entirely on market variables and prices. In the US context, this might make sense. By contrast, in Germany, cartellization was too far advanced for prices alone to serve as reliable indicators of economic conditions. Furthermore, in the early 1920s, monetary conditions were too disturbed. An adequate analysis of the German business-cycle had, therefore, to take into account "real" variables, such as production and employment.

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4 For the background to these reports see P.C. Witt, 'Bemerkungen zur Wirtschaftspolitik in der "Uebergangswirtschaft" 1918/1919' pp. 79-96 in D. Stegmann, B.J. Wendt and P.C. Witt eds. *Industrielle Gesellschaft und politisches System* (Bonn, 1978).

5 The term first appeared in the report for July 1921, see Bundesarchiv Koblenz R 43 I/1147 No. 548 ff.

6 U. Roeske, "Die amtliche Statistik des Deutschen Reichs 1872 bis 1939. Historische Entwicklung, Organisationsstruktur, Veröffentlichungen", *Jahrbuch für Wirtschaftsgeschichte* 1978, IV, pp. 85-107.

7 BAP Reichskanzlei 07.01 Film 19065N/2110 No. 3 Präs. SRA to Reichskanzlei 3.10.1924 and No. 8 Reichskanzlei to SRA 8.10.1924.

8 J.M. Keynes, *The Economist as Saviour* 1920-1937 (London, 1992), pp. 102-6

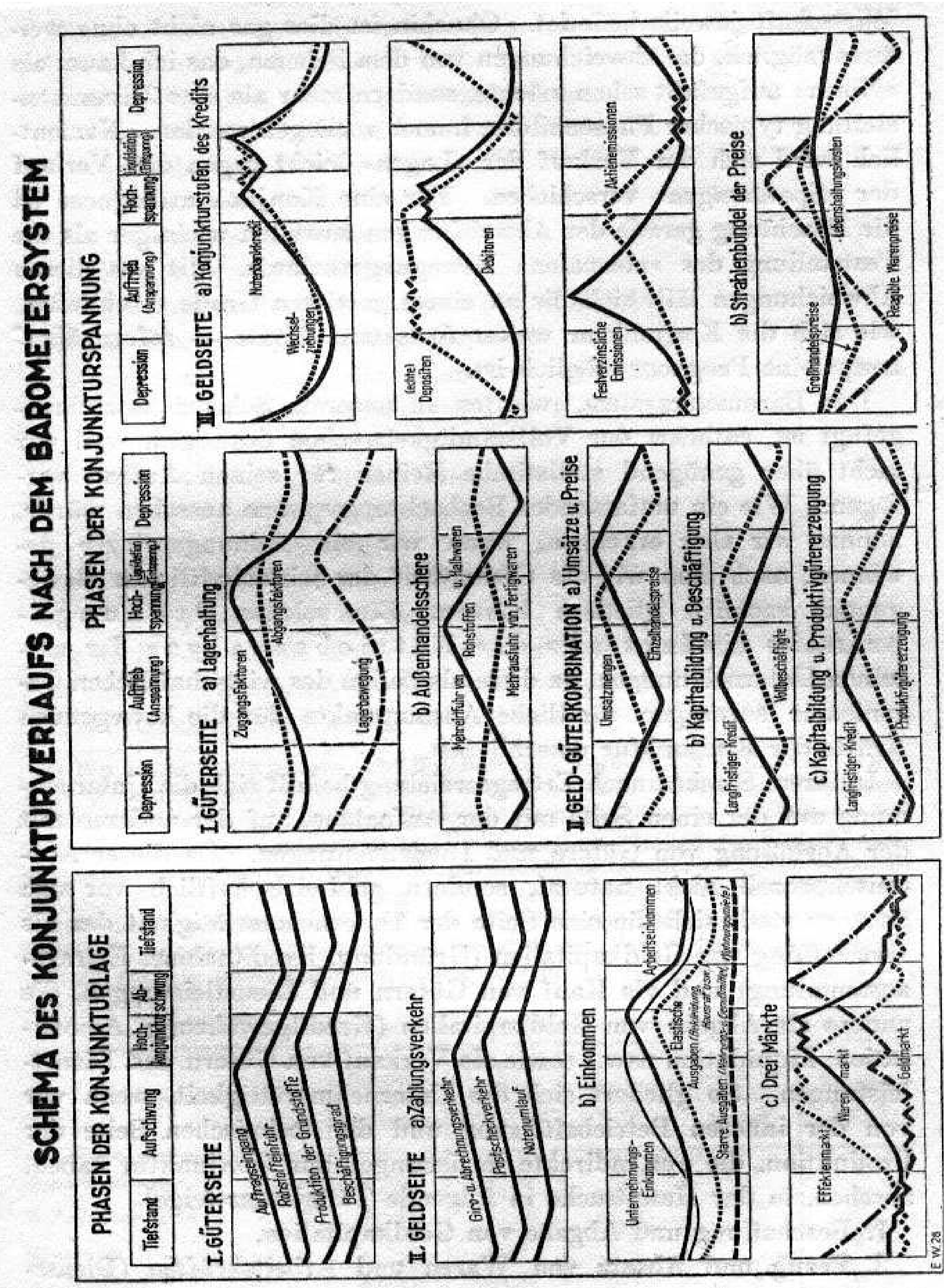
9 *Vierteljahrshefte zur Konjunkturforschung* 1 (1926), 1, pp. 4-5.

In 1925, Wagemann's ambition took on institutional form. In July, in the conference hall of the Statistical Office he called into existence the Institute for Business-Cycle Research[10]. Representatives of all levels of government, German business and trade union organisations attended the meeting. Over the following years, the Statistical Office and the Institute were to elaborate an entirely new apparatus of short-term economic observation. A dense network of surveys was established to monitor short-term fluctuations in unprecedented detail and breadth. A helpful summary of the system was provided in the Konjunkturlehre, a textbook of business-cycle economics, which Wagemann completed in September 1928. The Harvard Committee's three curve barometer was still part of the Institute's diagnostic system. However, it was now contextualized in a far more elaborate network of indicators. Out of this system of indicators, the Institute distilled the stylized image of the cycle shown in Figure 1[11].

[10] Geheimes Staatsarchiv Dahlem I Rep. 120 C VIII 2a Nr. 33 Bd. 1 Bl. 2 Protokoll über die am 16.7.1925 stattgehabte Sitzung zwecks Gründung eines Instituts für Konjunkturforschung.

[11] E. Wagemann, *Konjunkturlehre* (Berlin, 1928), p. 141. Fayolle in his paper in this volume reproduces a French translation of the IfK's barometer system (excluding the thermometers) taken from E. Wagemann, *Einfuehrung in die Konjunkturlehre* (Leipzig, 1929).

Figure 1: The IfK's Stylized Image of the Business Cycle



In this array, Wagemann reinterpreted the Harvard barometer (II.c.) as one of four so-called thermometers (I. & II.a, b, c), describing the state of the economy over the course of the cycle (Konjunkturlage)[12]. These thermometers he contrasted to business-cycle barometers, which registered what he referred to as 'cyclical tension' (Konjunkturspannung), the 'incongruencies, disturbed relations and tensions', which determined the dynamic of the economy. The 'Three Market Thermometer' described the sequential movement of prices in three key markets: the market for shares, the market for commodities and the money market. This, however, was just one was of summarizing the state of economic activity. The Institute gave three other thermometers equal priority. The 'real economy', the sphere of production, was represented by a thermometer consisting of four series (I), which also moved in sequence: an index registering the flow of new orders recorded by the trade associations; followed by a series reporting the import to Germany of raw materials necessary to feed an increase in production; an index of production compiled by the Institute; and finally an index of the level of employment, based on trade union reports. According to the Institute, the index of new orders was an even more sensitive indicator of the cyclical upswing, than the A-curve for shares in the Harvard barometer. The thermometer of payments activity (II.a) traced the sequential movement of three monetary indicators: the volume of cheque clearances, the volume of postal cheques and the volume of cash in circulation. Finally, what was probably the most important thermometer, (II.b), summarized fluctuations in income and expenditure. It contrasted the movement of profits and wage income and related these to the flow of expenditure on elastic and inelastic consumption.

Driving the fluctuations of these thermometers were a swarm of business-cycle barometers displayed on the right-hand side of the diagram. These too were divided into barometers monitoring the development of the real economy (I a, b), those which referred to the 'money side' (III a, b) and those which combined the 'monetary sphere' and the 'real economy' (II a, b, c). The dynamics of production showed up in two barometers. The first of these (I.a.) counterposed the forces contributing to stock-building – represented by indices for production, imports of raw materials, employment and bank overdrafts – to the forces tending to reduce the level of stocks held by German business – represented by indices for retail turnover, exports and the circulation of cash. According to the Institute, the interaction of these two elements meant that the level of stocks lagged the cycle, thus contributing powerfully to the dynamic both of the upswing and the downswing. In a workshop economy like Germany, a second set of forces driving economic fluctuations related to the trade account. Barometer I.b., therefore, compared the trade surplus in manufactured goods with the deficit in raw material and semi-finished products. During the upswing, the overall balance of trade would tend to become unfavourable, as imports increased and the exports decreased. During the phase of depression, the reverse effect occurred. Overall, therefore, the Institute attributed a dampening effect to Germany's foreign trade account.

The 'money side of the economy' was represented by four barometers. Three of these related to the credit market. One monitored the relationship between bills of exchange issued by the private banks and credits provided to them by the Reichsbank. This exhibited a characteristically jagged pattern at the upper turning point as credit conditions tightened and pressure on the central bank rose to a maximum. A second barometer counterposed new overdrafts granted and new deposits received by the ten largest German banks. This was designed to reveal the liquidity building up in the banking system during the depression phase of the cycle, which supported the rise in borrowing in the upswing. A third credit barometer compared the relative movement of bond and new share issues, with bond issues peaking early in the upswing and new issues of shares reaching their peak close to the upper turning-point. The last of the monetary indicators, (III.b.), contrasted the relative movement of prices for goods. An index of reactive prices was chosen to reflect those

12 E. Wagemann, *Konjunkturlehre*, p. 140.

prices that were free of cartel restrictions and highly responsive to the business cycle. This led a revised version of the official index of wholesale prices. The official index of the cost of living trailed along behind.

To combine the "money side of the economy" with the "goods side", the Institute proposed three further barometers, which completed its diagnostic system by combining price and quantity indices. The first of these, (II.a), counterposed the volume of turnover with the level of retail prices. According to the Institute, it was movements in the volume of sales that drove adjustment in the price level. The second 'combined barometer', (II.b), compared the value of long term loans issued to German business with the level of employment. In this case, it was new loans that preceded the upturn in employment. Finally, barometer II.c contrasted the value of long-term loans with an index of the output of so-called production goods. The last two barometers were significant because they recapitulated the attempt by the well-known university economist Arthur Spiethoff to construct a barometer based on the relationship between the production of iron and steel and the new issue of shares. Like the Harvard barometer, Spiethoff's chart was able to capture an interesting relationship, between capital investment and heavy industrial production. But as experience had showed, such limited barometers provided an unsafe basis for prediction. The aim of the Institute was to achieve a more comprehensive insight into the dynamics of the macroeconomy, by placing a variety of such partial indicators in relation to one another.

For the short period between 1926 and 1929, the barometer system did indeed seem to provide a good guide. In May 1926, the flow of new orders reported by trade associations allowed the Institute to anticipate the trough of Weimar's first recession. And the barometer system also anticipated the pronounced slowdown in the recovery at the end of 1927. Based on this experience, Wagemann was emboldened to claim that the Institute could make forecasts of overall economic activity for at least three months ahead[13].

2. Ernst Wagemann's Macroeconomics

Clearly, the IfK's index was a substantial modification of the Harvard barometer. But was there a logic driving this proliferation of tables and indices? Many contemporaries were sceptical. They regarded the expansion of the Reich's statistical system as mere ad hocery, driven as much by administrative empire-building as by any clear-cut intellectual agenda[14]. Academic critics, including Adolph Loewe, who had left the Institute in 1926 to accept a Professorship at the Kiel Institute of International Economics, jumped to their guns, accusing Wagemann and his economists of an outright rejection of economic theory[15]. The Institute was tarred with the brush of "American empiricism". Those who preferred to find their enemies at home cast Wagemann as the naive historical economist in a rerun of the Methodenstreit[16]. And subsequent commentators have largely echoed these views. Those few who have spent time on the history of the Institute have tended to treat it as little more than a German variant of the Harvard Committee[17].

However, these assessments rest on a serious misreading. To focus on the intricacies of the barometer system is to miss the point. What made the Statistical Office and the Institute truly distinctive was their determination to abstract from individual markets and price movements, so as to be able to study the economic

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13 E. Wagemann, *Einfuehrung*, p. 133-136.

14 H. Kuschmann, *Die Untersuchungen des Berliner Instituts fuer Konjunkturforschung. Darstellung und Kritik* (Jena, 1933).

15 A. Loewe, 'Wie ist Konjunkturtheorie ueberhaupt moeglich?', *Weltwirtschaftliches Archiv*, 24 (1936), pp. 165-196.

16 See the critical views of Diehl expressed in K. Diehl (ed.), *Beiträge zur Wirtschaftstheorie. Erster Teil: Volkseinkommen und Volksvermögen. Begriffskritische Untersuchungen. Schriften des Vereins für Sozialpolitik* (SVS) 173 I (Munich, 1926) and *Zweiter Teil: Konjunkturforschung und Konjunkturtheorie* 173 II (Munich, 1928).

17 E. Coenen, La '*Konjunkturforschung' en Allemagne et Autriche 1925-1933* (Paris, 1964) and B. Kulla, *Die Anfänge der empirischen Konjunkturforschung in Deutschland* 1925-1933 (Berlin, 1996).

process in the aggregate. The basic abstraction was to view the national economy, not as a collection of firms, or a collection of unconnected markets, but as an organic, interconnected, Kreislauf (circular flow) of money and goods. The driving force behind this research programme was Ernst Wagemann. This too needs to be emphasized. Wagemann has not hitherto been taken seriously as an economist[18]. Certainly, Wagemann was no Keynes. He was not brilliantly original. Nor was he a rigorous theoretician. He did not handle mathematical abstraction with confidence. He is easily overshadowed by more illustrious and original contemporaries in the German-speaking world, men such as Schumpeter or Hayek. However, early on in his intellectual development Wagemann seized upon one big and powerful idea. He realized the possibilities inherent in basing both economic theory and empirical economic research on a holistic, aggregative understanding of the economy. And, unlike many of his more brilliant colleagues, Wagemann did not suffer frustration, marginalisation or emigration. After 1924, he was able to put his idea to work as the basis for German official economic statistics.

The seminal statement of Wagemann's economics is to be found in his little-regarded *Treatise on Money*[19]. The first and only published volume of this work was completed in May 1923. The purpose of the Treatise was to affect reconciliation between the most influential strands of monetary thought in the German and English-speaking worlds: the nominalism of Knapp on the one hand and the Quantity Theory as reformulated by Irving Fischer. Wagemann's reformulation of nominalism need not detain us here, for the core of his economic argument was addressed to the Quantity Theory. By espousing the Quantity Theory, Wagemann believed that he was rejecting an atomistic approach to the study of economics. According to Wagemann, the analysis of monetary exchange could not start with abstract individuals and their interactions in the market place, as proposed by the marginalists. What the Quantity Theory of Money provided was a rudimentary theory of the economy in the aggregate. The value of the unit of money was determined with reference to the entirety of all transactions. Individual preferences and the actions that supposedly followed from them were submerged in a simple piece of algebra.

$MV = PT$

The stock of money (M) multiplied by the "velocity" (V) of its circulation was equal to the total number of transaction (T) times the average price level (P). The problem was how to quantify abstract concepts like the velocity of circulation? How could one count the total stock of money or the total volume of transactions? These were important questions for Wagemann. He was highly critical of theorizing that was incapable of factual substantiation. Characteristically, Wagemann championed the work of Irving Fischer, a choice that put him at odds with most of his German contemporaries. Fischer had made the first serious effort to put empirical flesh on the bones of the Quantity Theory using a variety of price indices and other proxy variables[20]. Wagemann's own ambition for Volume Two of his *Treatise on Money* was to complete a similar empirical analysis for the German economy.

However, despite his enthusiasm for Fischer's work, Wagemann was sceptical about the basic equation of the Quantity Theory. The variables contained in the equation of exchange were hard to operationalize and historically contingent on the development of the financial system. In his *Treatise*, Wagemann set out to demonstrate how a new equation of exchange could be built on 'categories, which have a more comprehensive

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18 See the dismissive judgements in B. Kulla, Die Anfänge, pp. 43-48 and R. Vilk, *Von der Konjunkturtheorie zur Theorie der Konjunkturpolitik. Ein historischer Abriß 1930-1945* (Wiesbaden, 1992), p. 186.

19 E. Wagemann, *Allgemeine Geldlehre* (Berlin, 1923). For an extensive and laudatory review see see H.S. Ellis, *German Monetary Theory* 1905-1933 (Cambridge, 1937).

20 I. Fisher, *The Purchasing Power of Money* (1911).

economic content'[21]. In making this move, he followed a small band of German-speaking economists who sought to modify the Quantity Theory by replacing its elusive variables with more meaningful and easily grasped concepts. At the centre of economic analysis, they proposed to place the concept of income[22]. Ultimately, it was income, expressed as purchasing power that determined the aggregate price level. Wagemann's immediate inspiration was Schumpeter's article, 'Das Sozialprodukt und die Rechenpfennige'[23]. Schumpeter proposed to transform the Quantity Theory by replacing the elusive monetary variables on the left hand side of the equation of exchange, by national income. Wagemann's Treatise developed this rather slippery idea into a comprehensive accounting framework, which spelled out the connections between prices, output, income and expenditure. Wagemann's so-called 'national economic accounts' ran as follows[24]:

(1) price * net output =

(2) production costs (material & personal) & profit =

(3) income =

(4) consumption + saving =

(5) (consumed and capitalized output) * price

With these five lines, Wagemann attempted to capture the basic circularity of economic activity. Starting at the top, line 1 registered the value of production. The original German refers to prices multiplied by a quantity of "utility effects" (Nutzeffekten). From the subsequent elaboration it is clear that what is meant is a measure of net output i.e. goods and services that were "directed" via the market towards consumption or net investment i.e. new stock-building or capital equipment. Line 2 recorded the way in which this value could be attributed to the "factors of production" as wages and profits. Wagemann distinguished production costs into personal costs (Persönliche Gestehungskosten), including wages, interest, entrepreneurial profit, rent, and material costs (Sachliche Gestehungskosten), i.e. raw materials etc. The importance of this distinction is that it introduced a division between households with which personal costs were incurred and firms with which material costs were incurred. On line 3, the total payments to factors of production were summarised as income. From the text, it is clear that Wagemann presumed all income ultimately to be distributed to households. The material costs of one firm became the "personal costs" of their suppliers, and so on. As line 4 stated, the total of income was either consumed or saved. The original text referred, somewhat misleadingly, to "Ausgaben" and "Ersparnis" i.e. expenditure and saving. However, from the accompanying text, it was clear that Wagemann actually had in mind the expenditure decisions of households (the ultimate recipient of all income). His explanation of the term "expenditure" made it clear that he was actually referring to final consumption of goods and services. Consumption and saving in turn had to be equal to the value of all goods produced either for consumption or net investment. There was no line recording total expenditure, as the sum of household consumption and investment by firms. However, Wagemann did not fail to draw the conclusion that there should be a "parallel" between saving in its various forms, including savings accounts, stocks and shares, mortgages etc., and new investment (Neubildung von Produktivgütern). He pointed out, however, that this identity has never been demonstrated empirically.

[21] E. Wagemann, *Allgemeine Geldlehre*, p. 146.

[22] See the discussion in H. Janssen, *Nationaloekonomie und Nationalsozialismus. Die deutsche Volkswirtschaftslehre in den dreissiger Jahren* (Marburg, 1998), pp. 274-306.

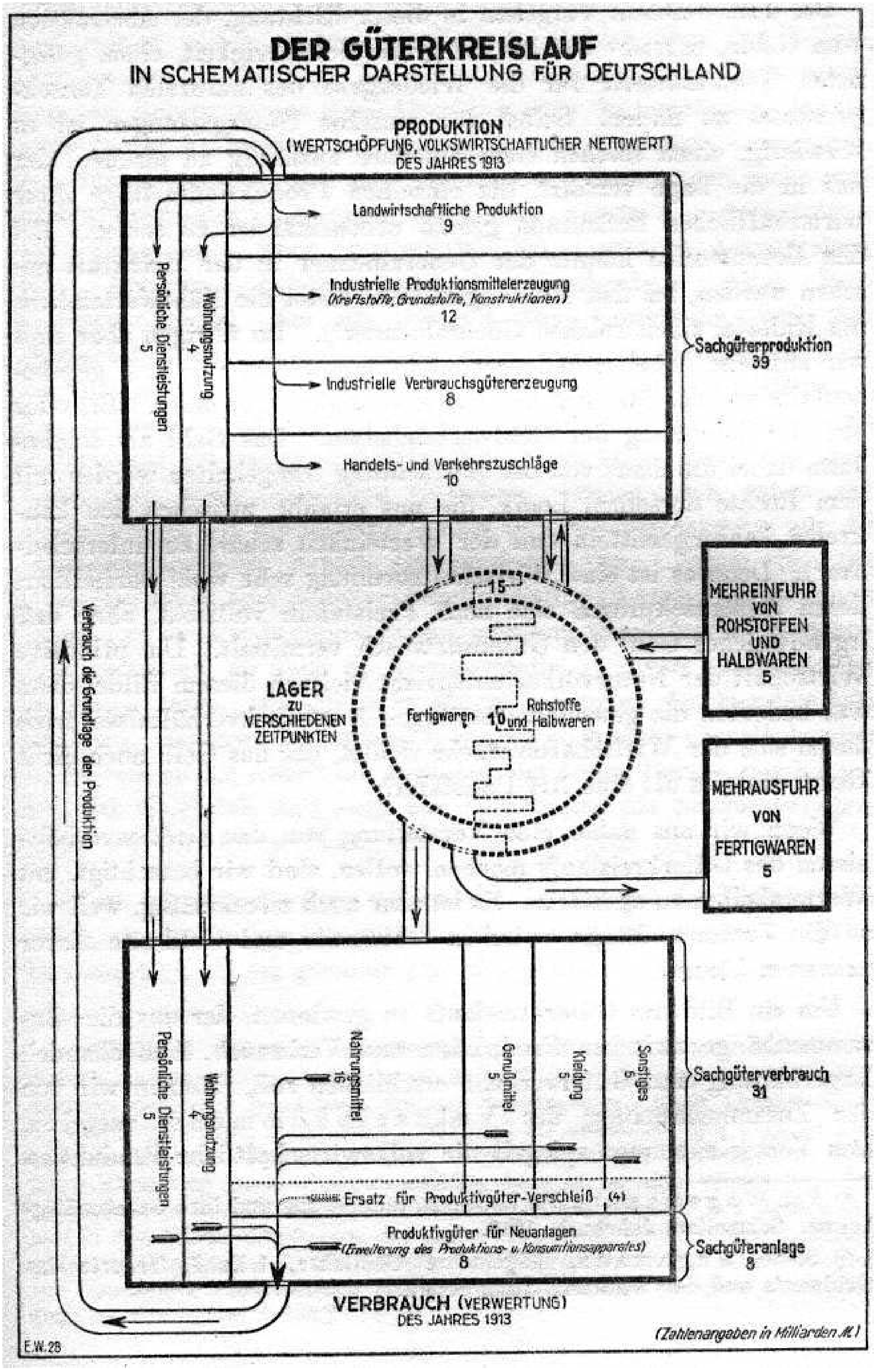
[23] J. Schumpeter, `Das Sozialprodukt und die Rechenpfennige', *Archiv für Sozialwissenschaft und Sozialpolitik* 44 (1918), pp. 627-715.

[24] E. Wagemann, *Allgemeine Geldlehre*, p. 150.

The elaboration of this set of equations concluded the theoretical section of the first volume of the *Treatise*. Wagemann's intention for Volume 2 was to apply this framework to an analysis of economic fluctuations over time. His appointment as President of the Statistical Office in the spring of 1924 deprived him of the opportunity to complete this work. According to Joseph Schumpeter, who knew Wagemann well, Wagemann found a substitute for his unfinished second volume in the output of the IfK and the SRA[25]. The early efforts at adapting the Harvard Committee's barometer system to Germany were no more than a stop-gap. The intellectual blueprint for the research of Germany's statistical establishment was laid down in the Treatise. The Konjunkturlehre, which appeared in 1928 as a summary of the Institute's work to-date, contained a striking graphical rendition of lines four and five of the accounting scheme in the *Treatise*[26]. Figure 2 shows what Wagemann referred to as the 'physical circuit of production' for the German economy in 1913, with statistical estimates for the major items of consumption and investment and the equivalent categories of production.

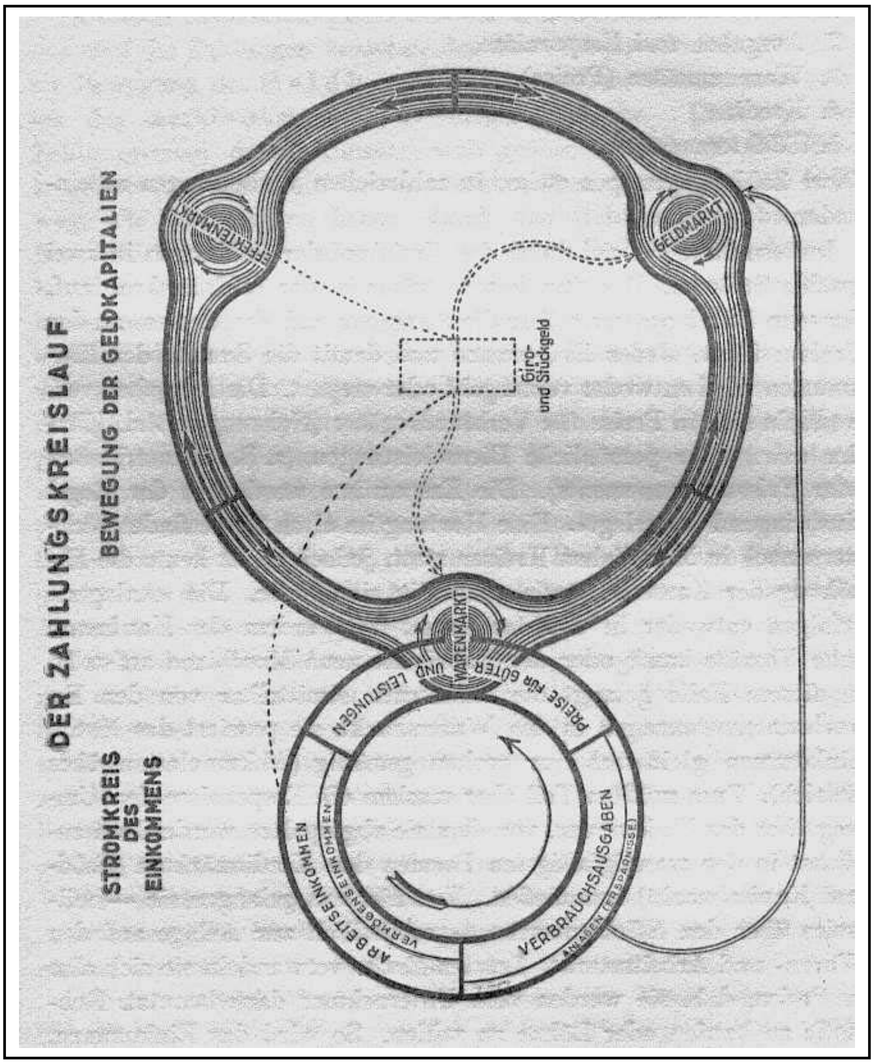
25 J. Schumpeter, *History of Economic Analysis* (New York, 1954), p. 1166.
26 E. Wagemann, *Konjunkturlehre*, p. 26.

Figure 2: The Physical Circuit of Production, Germany 1913, bn RM



Lines one through four of the national accounting scheme in which the value of output is connected to flows of income and expenditure were represented in the rather more abstract diagram reproduced as Figure 3. This was based on the circulation of money, rather than the physical circuit of production and linked the circulation of money between producers and consumers (on the left-hand side of the diagram) to the expanded circuit of money capital (on the right).

Figure 3: Wagemann's Circuit of Payments:



The aim here is not to argue that Wagemann invented the concept of the circular flow, or was the first to represent it in the form now so familiar from economics textbooks. In 1920s Germany, Wagemann was just one of a number of economists whose thinking revolved around the metaphor of the Kreislauf[28]. And as Wagemann, Leontief, Plenge, Lederer, Schumpeter and others were only too well aware, this conception of the economy could be traced back, via Marx, to 18th century precursors, above all Quesnay[29]. What is important in the present context is simply to establish that it was this holistic, macroeconomic conception that underpinned the work of the Institute and the Reich's Statistical Office in the mid-1920s. The Weimar Republic can thus be reckoned, alongside the USA and the Soviet Union, as one of the states to have pioneered modern macroeconomic statistics.

27 E. Wagemann, *Konjunkturlehre*, p. 41.

28 E. Lederer, `Der Zirkulationsprozess als zentrales Problem der ökonomischen Theorie', *Archiv für Sozialwissenschaft und Sozialpolitik* 56 (1926), pp. 1-25.

29 J. Plenge, `Zum 'Tableau Économique'', *Weltwirtschaftliches Archiv* 24 (1926), pp. 109-129.

3. Indicators and National Accounts

With this macroeconomic image of the economy in mind, the baroque indicator system of the Institute gains a new coherence. Amongst the thermometers that registered the state of the economy, the Institute gave pride of place to a set of figures, recording the fluctuation of incomes and expenditure over the cycles. Wage income and profits, in turn, were 'functionally related' to the level of real economic activity on the 'goods side', i.e. production and employment. The purpose of the various barometers, both for the 'real economy' and the 'money side', was to explain the fluctuations in production, income and expenditure. Following the accounting scheme of the *Treatise*, Wagemann and the Institute defined the business-cycle not as the fluctuation of prices, but as the fluctuation of aggregate economic activity. According to the *Konjunkturlehre*, 'fluctuations in national economic turnover' were 'the most comprehensive expression for the movement of the business-cycle'[30]. Since they were not susceptible to direct measurement, in the absence of short-run national income statistics, the Institute instead focussed its attention on employment and wage income. Diagnosing and predicting the level of aggregate economic activity, as represented by the state of employment, was the central purpose of the Institute's indicator system.

To our eyes, the choice of employment as the key variable of analysis may seem obvious. The state of the labour market was after all an issue of key political significance to the Weimar Republic. With the birth of Keynesian analysis, unemployment was to become, for almost half a century, the central preoccupation of macroeconomics. However, the labour market did not form the first object for business-cycle analysis. In 1900, the administrative infrastructure necessary to register unemployment had, in most countries, still to be created. Furthermore, the straight-jacket of equilibrium theory made unemployment a variable with dubious theoretical connotations. The very idea of involuntary unemployment opened the door to subversive theories of underconsumption. The very first analyses of the business-cycle focussed not on "real variables" such as employment or even production, but on prices. Around the turn of the century, when more comprehensive accounts of the cycle did begin to emerge, it was the dramatic fluctuations in the production of commodities such as steel, which caught the eye. Arthur Spiethoff chose to build his widely-read historical account of the German business-cycle around the correlation between fluctuations in stock prices and the production of iron and steel[31]. In rejecting a description of the cycle either in terms of prices or in terms of a particular product market, Wagemann's Institute was marking a break with the mainstream. The cycle was an aggregate phenomenon and had to be described in aggregate terms. Faute de mieux, labour provided a basic common denominator of all economic activity.

The labour market also provided a metric for the business-cycle. It allowed the Institute to move from an impressionistic qualitative assessment of the pattern of economic development and the functional relations between different variables, to a basic quantitative approach[32]. Fluctuations in unemployment gave at least some idea of the true amplitude of fluctuations in aggregate economic activity. In the pre-war period, the rate of unemployment amongst unionized workers in German industry had risen to between 4 and 5 percent in the worst months of 1908 and 1913. These figures, of course, included a large numbers of seasonal workers. Furthermore, unemployment amongst unionized workers exceeded that in other sectors. Wagemann therefore reckoned that the pre-war cycle could have accounted for a fluctuation of at most 2-3 percent in total employment[33]. In 1926, the worst postwar year, Wagemann estimated that cyclical unemployment might

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30 E. Wagemann, *Konjunkturlehre*, p. 196.

31 A. Spiethoff, '*Krisen' in Handbuch der Staatswissenschaften* (4th ed., 1925), Vol. 6, pp. 8-91.

32 The shift from qualitative to quantitative analysis is made explicit in E. Wagemann, *Einfuehrung*, p. 108.

33 E. Wagemann, *Konjunkturlehre*, p. 195-196.

have reached 10 percent of the total workforce. The significance of this basic measurement of the amplitude of the cycle is evident when we compare fluctuations in employment to fluctuations in the production of heavy industry, which was Spiethoff's chosen index. During 1908, the production of raw iron plunged by over 20 percent and it fell by an even larger percentage during the 1925-1926 recession. Iron was a sensitive measure of cyclical fluctuations. The Institute was happy to incorporate it into its predictive barometers. But it gave a misleading impression of the scale of cyclical fluctuations in the aggregate.

What determined the level of employment? Assuming that wages, the productivity of labour and the structure of production were given in the short-run, the basic determinant of employment was the volume of production, which in turn was functionally linked to demand. Employment was thus linked directly to Wagemann's national accounting scheme. In principle, fluctuations in employment should therefore be related to fluctuations in aggregate production, expenditure and income. In its first quarterly report, the Institute provided an analysis of the violent cyclical downturn of 1925-1926 in precisely these terms. In 1925, the Institute estimated a lower bound for German national income of 50 bn RM[34]. Of this, 25 bn RM was relatively inelastic expenditure on food. A further 5 bn went on housing and rent. The variable items, therefore, included 5 bn RM public spending, savings of 5 bn RM which provided the funds for investment and 10 bn RM spent on consumer durables such as clothing and household equipment. Over the course of the cycle, the IfK estimated that government spending had fallen by 1 bn Rm. Saving had been reduced by 2 bn RM and expenditure on clothes and household goods had fallen by 2 bn RM[35]. The result was an extremely uneven cyclical downturn. Consumer durable purchases were reduced from 10 to 8 bn RM. Investment had fallen from 5 bn RM to 3 bn RM. The sectors supplying capital goods and consumer durables, therefore, suffered a savage cyclical contraction, whereas the rest of the economy was relatively little affected.

The national income figures that the Institute used for 1925 were no more than rough estimates. Work on more precise statistics began in earnest in the autumn of 1926[36]. Initially, the statisticians concentrated on 1913, preparing a revised estimate of Karl Helfferich's figures on the basis of Prussian and Saxon income tax figures. By the autumn of 1928, when he completed the text of his *Konjunkturlehre*, Wagemann was able to present a comprehensive national account for 1913. Income was divided into different classes of earnings. What is more, the *Konjunkturlehre* also contained estimates for production, decomposed into the main sources of value added. Expenditure was also clearly classified. And the interrelation between the different element was summarized in the diagram of circular flow reproduced above. By 1932, the Reich's Statistical Office felt sufficiently confident to publish the first official figures for national income to appear for any major Western European country[37]. The history of the pre-war business-cycle was rewritten in macroeconomic terms. National income figures were adjusted for year on year inflation. Given the sustained growth of the German economy after 1890, the Institute had to contend with the familiar problem of growth cycles. Between 1890 and 1914, there was no year in which national income had actually fallen. The cycle thus expressed itself in fluctuations in the growth rate of real income around the trend of 1.8 percent per annum[38]. The amplitude of these fluctuations was 2-3 percent at most, far smaller than the fluctuation in the level of wholesale prices.

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34 VzK Vol. 1 (1926) Heft 2, p. 21.

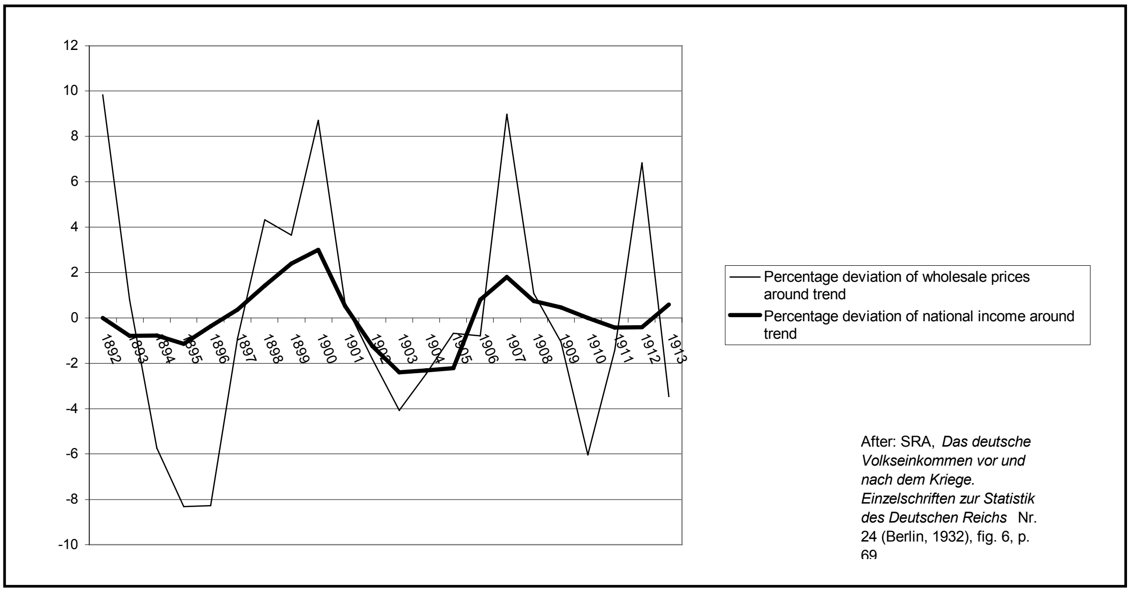
35 The Institute left unspoken the fact that a reduction in saving was assumed to imply an equivalent reduction in investment. A worked-out theory of the multiplier was absent from the work of the Institute in the 1920s.

36 J.A., Tooze, *Statistics*, p. 122-128.

37 SRA, *Das deutsche Volkseinkommen vor und nach dem Kriege* (Berlin, 1932).

38 SRA, *Das deutsche Volkseinkommen*, pp. 67-69.

Figure 4: The Cyclical Fluctuations of Pre-war National Income and Prices



The expenditure side of the national accounts could not be approached in the same direct fashion. Investment, public expenditure and consumption each required separate methodologies. An analysis of investment was begun in the late 1920s and published in 1931 under the auspices of the IfK[39]. Keiser and Benning's study was driven in part by structural questions about the reconstruction of German capital after war and hyperinflation. And it was these issues, which aroused the public controversy that surrounded the report. However, Keiser and Benning's figures also clearly showed the role of industrial investment and stock building in cyclical fluctuations. Both elements of capital formation had plunged in 1926 before recovering sharply in 1927 and 1928. Organizing the public accounts to suit the needs of economic analysis involved a mixture of political finesse, accounting skill and conceptual development. The task fell to Dr Gerhard Colm, who was also responsible for the national income estimates. The elaborate treatment of the public sector in the national income accounts was to be one of the distinctive achievements of German official statistics in the interwar period[40]. Consumption remained the component of expenditure that was least well understood. A study of working class household budgets was conducted over the course of 1927. But, this captured only a fraction of total household expenditure. To remedy the defect, regular surveys were taken of the turnover of all branches of retailing.

By the late 1920s, the lack of comprehensive data on production was the only glaring gap in the Reich's statistical repertoire. Since the turn of the century Anglo-American statisticians had pioneered the techniques of the industrial census. Their starting point was the concept of National Dividend as defined in the last quarter of the 19th century, by Alfred Marshall. In his classic textbook, the *Principles of Economics*, Marshall provided both a definition of National Dividend, a methodology for calculating it and the demonstration in theory that net production should be identical to the value of national income[41]. All-encompassing censuses of production along these lines had been carried out in the US since the mid-19th century, the British Dominions since

[39] G. Keiser and B. Benning, *Kapitalbildung und Investitionen in der deutschen Volkswirtschaft 1924 bis 1928* IfK Sonderheft 22 (Berlin, 1931).

[40] G. Colm, *Volkswirtschaftliche Theorie der Staatsausgaben* (Tuebingen, 1927).

[41] A. Marshall, *Principles of Economics*, (8th edition, London, 1938), 79-8, 523-4, 827. Significantly, Schumpeter, who made use of the concept of National Dividend in his own work, credited Marshall not only as the great formalizer of the marginalist engine of analysis, but also as a pioneer of 'aggregative analysis', s. J.A. Schumpeter, *Ten Great Economists from Marx to Keynes* (London, 1952), p. 106.

the 1890s, and in Great Britain since 1907. And there was no doubt that the basic procedure was sound. As the NBER was able to show for the United States in the early 1920s, estimates of national production prepared on the basis of the production census agreed with independently estimated figures for national income to within a very small margin of error[42].

Clearly, estimating the value of German industrial production had to be an integral part of the programme of Konjunkturforschung. Unfortunately, Wagemann's staff had little to work with. Germany statistics of industrial production were scrappy. Their scope was defined largely by administrative convenience, business resistance and the narrow concerns of trade policy. Similar considerations dictated the range of questions. In total they covered no more than a small fraction of German industry. By 1927, this no longer satisfied the ambitions of the IfK. To verify Wagemann's equations and to complete the national accounting scheme, the Institute needed a figure for value added. Without it, the entire edifice of production indicators was based on little more than rough estimates and proxies. Furthermore, deciphering the connections between the branches of German industry was vital to understanding the propagation mechanism of the cycle. How did shocks spread from agriculture to industry, from exporting sectors to the rest of the economy, and from investment goods industries to the producers of consumer goods? By the late 1920s, the Institute was already piecing together a rough picture of these interconnections from cost accounts. In 1927, Wagemann ordered his staff to prepare plans for Germany's first census of industrial production, which would reveal the entirety of these input-output relationships[43]. The first count was to cover the year 1930.

The Great Depression intervened to frustrate the completion of Weimar's national accounting scheme. As part of Chancellor Bruening's effort to balance the budget, all new statistical work, including the census of production for 1930 was cancelled. In 1933, however, just before Hitler's seizure of power, the census was put back on the agenda and funds were allocated for the work to be completed in 1934. By this time Wagemann had been removed as President of the Statistical Office and the Institute had been separated from the main body of official statistics. However, the macroeconomic conception of official statistics lived on within the Reich's Statistical Office. The production census of 1933 was carried out as a trial run for a more comprehensive evaluation that was taken in 1936 and published as the much-quoted volume, *Die Deutsche Industrie*[44]. This presented the first comprehensive evaluation of German industry. For the first time, it revealed the crucial ratios between turnover and value added, that allowed estimates of German industrial production to be incorporated into the national accounting scheme and placed the monthly and quarterly indices of production on a sound basis. Meanwhile, estimates of German national income, continued to be published regularly until 1938[45]. During the 1930s, the SRA also began to experiment with short-run estimates of national income, including quarterly data for the wage bill and corporate earnings[46]. These were published by the SRA, even though the gap between rising employment and the wage bill revealed the artificiality of the first phase of Hitler's recovery.

The IfK for its part, though it was cut off from the main source of official information, did not lie idle[47]. It continued to publish its quarterly analyses of the German economy. At least until 1936, these continued to be

42 G. Alchon, *The Invisible Hand of Planning. Capitalism, Social Science and the State in the 1920s* (Princeton, 1985), p. 59-63.

43 The plans were prepared by Dr Wilhelm Leisse and his junior colleague Herr Dorn between 1927 and 1928. See BAP 31.02 6181 "Die industrielle Produktionsstatistik im In- und Ausland" (ORR Leiße, Ref. Dorth) (1927/8).

44 Reichsamt fuer Wehrwirschaftliche Planung, *Die deutsche Industrie. Gesamtergebnisse der amtlichen Produktionsstatistik* (Berlin, 1939).

45 For a graphical presentation of the cyclical fluctuations in national income between 1925 and 1937 see *Wirtschaft und Statistik* 18 (1938), pp. 802-805.

46 *Vierteljahrshefte zur Statistik des Deutschen Reiches* 42 (1933), pp. 112-120 and *Vierteljahrshefte zur Statistik des Deutschen Reiches* 43 (1934), pp. 69-77.

47 J.A. Tooze, Statistics, p. 177-183.

phrased in the language of the business-cycle. A variety of short-term monthly and quarterly indicators were used to track the course of economic development. However, these were no longer formally grouped into barometers. Instead, the basic idea of a circuit of income, expenditure and production was brought every more clearly into the forefront. Ideological constrains were minimal. In 1936, the Institute ran into zealous opposition from a number of subordinate Nazi agencies for speculating about the likely prospects for an economic downturn. On the basis of a normal cyclical periodicity, there was every reason to expect a slowdown in the recovery in 1936. However, in light of the intensification of economic controls in the form of Goering Four Year Plan 'business-cycle pessimism' was politically unacceptable. Wagemann's Institute was forced into a tactical retreat, but was never really endangered by these attacks. It was too useful to a variety of institutions in the regime and its reports remained the standard source of information for both internal and external observers of the German economy right up to 1939.

4. From Observation to Control

There was no doubting, however, that the Nazi regime's increasingly comprehensive controls of the German economy were changing the terms of the debate. What role was there for Konjunkturforschung under these new conditions? Observation of the economy as an independent object governed by its own logic of development was clearly no longer an attractive strategy, either rhetorically or in practice. To survive the Institute was forced to supplement general economic observation with contract research for institutions such as the German post office. By contrast, the staff recruited by Wagemann into the Reich's Statistical Office sensed more exciting opportunities. For a number of entrepreneurial members of the Statistical Office it seemed that the tools of macroeconomic Konjunkturforschung could now be refashioned as means of economic control[48]. And the censuses of industrial production, originally designed as tools of cyclical economic analysis provided the obvious launching pad.

Despite their roots in Konjunkturforschung, the production census of 1933 and its successor in 1936 were justified to the political authorities not as exercises in national accounting but as contributions to Germany's military-economic planning (Wehrwirtschaft). The censuses, after all, provided an unprecedented volume of information on the physical input-output relationships of the economy. For the statistician chiefly in charge of the survey, Dr Wilhelm Leisse, the censuses became the launching pad for a career, which by 1938 was to put him in charge of his own rival to the Reich's Statistical Office, the Reich's Office for Military-Economic Planning (Reichsamt fuer wehrwirtschaftliche Planung, RwP). In 1938, this office was charged with the mission of preparing mobilization plans for German industry. For all key raw materials, the RwP was to prepare balances matching the quantities demanded by German industry against the available supply. The result was a staggering array of physical input-output ratios, graphically depicted in so-called 'family tree diagrams'. However, Leisse's efforts also revealed the inherent limitations of a physical accounting system. It was virtually impossible to compile effective raw material balances for the manufacturing sector, which depended on an overwhelming variety of inputs, that could not be aggregated in physical terms.

However, physical accounting was not the only possible interpretation of Germany's industrial censuses. Under Dr. Paul Bramstedt, one of Wagemann's chief collaborators in the 1920s, the economic statistics department of the Statistical Office proposed a different interpretation. Relying on prices to allow aggregation, Bramstedt proposed that the production census should be used to produce a comprehensive Input-Output table for German industry[49]. This proposal coincided with the publication in the United States of Wassily

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48 The following section summarizes J.A. Tooze, Statistics, chapters 5-7.

49 The only hint of this work to reach the public came in P. Bramstedt, 'Gefuege und Entwicklung der Volkswirtschaft', *Allgemeines Statistisches Archiv*, 25 (1936/1937), pp. 377-404. Professor Fremdling at Groeningen University is currently attempting to reconstruct an Input-Output scheme for German industry in 1936, using the census results.

Leontief's famous first experiments in Input-Output accounting[50]. And this was no coincidence. On his way from Russia to the United States, Leontief had spent a number of years in the Weimar Republic. During his time in Berlin and Kiel he had reviewed the early Soviet work in national accounting and experimental input-output tables[51]. These activities were also eagerly followed by researchers in Wagemann's Institute including both Paul Bramstedt and the young Rolf Wagenfuehr[52]. Like Leontief, Bramstedt presented his proposal for an Input-Output scheme as a logical extension both of business-cycle research and the parallel efforts to compile a system of net accounts for national income, production and expenditure. The net estimates of production and income could be continuously checked by the complete breakdown of gross industrial turnover and its interconnections. Meanwhile, the production of regularly updated Input-Output tables would allow the process of fluctuation to be traced back to its sectoral routes. For the inheritors of 1920s Konjunkturforschung, the transition from barometer systems to the most comprehensive national accounting was seamless. In practice, however, Bramstedt's Input-Output table ran into problems. Secrecy restrictions and bureaucratic rivalry with Leisse's RwP inhibited the necessary exchange of information. As far as I am able to establish from the archival record, the only concrete result was a rough table based on the 1933 census, of which only a sectoral account for the motor vehicle industry has survived. In technical terms, Bramstedt's Input-Output proposal was clearly to be preferred to the naïve model of physical planning expounded by Leisse and his RwP. However, unlike Leisse, who could count on the support both of Goering and the military, Bramstedt lacked allies.

In the late 1930s, the German statistical apparatus entered a period of infighting and self-destruction. Not only was the scheme for an Input-Output table stifled, but the production census planned by Leisse for 1939 was cancelled as well. Despite more than a decade of effort and numerous extremely promising proposals, Nazi Germany entered the war without a functioning economic information system. But, the exponents of Konjunkturforschung were still not dead. Ironically, the separation of Wagemann's Institute from the Reich's Statistical Office in 1933 preserved it from the bitter infighting, which afflicted the apparatus of official statistics in the late 1930s. The Institute stood ready, therefore, to re-enter the fray during the war.

The key sector in need of urgent reorganization was the coal industry. Coal supplied over eighty percent of Germany's energy needs, not to mention the raw material for the synthetic fuel program. Wagemann's Institute was contracted in 1941 to create a new statistical control system for this vital sector. Characteristically, the Institute treated the problem of coal supply as more than merely an administrative issue. For Wagenfuehr, energy input was a common denominator of industrial production. Whilst the pre-war systems used for recording output were rendered useless by the wholesale conversion to wartime production, an index of energy consumption provided a useful proxy to monitor the short-term movements of both civilian and military production[53]. Success in dealing with coal brought Wagenfuehr and his team to the attention of one of the leading personalities in the Reich's Ministry of Economic Affairs, Hans Kehrl. Kehrl had been an enthusiast for the rationalization movement since the 1920s. He had visited the USA to witness the miracle of modern mass production at first hand. He was also an avid reader of the Institute's quarterly barometers

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50 W. Leontief, 'Quantitative Input and Output Relations in the economic System of the United States', *Review of Economic Statistics*, 18 (1936), pp. 105-125. A bibliographical reference to this article was kept on file by Bramstedt's department, see Bundesarchiv Lichterfelde 31.02 SRA 2705 no. 56.

51 W. Leontief, 'Vom Staatsbudget zum einheitlichen Finanzplan. Sowjetrussische Finanzprobleme', *Weltwirtschaftliches Archiv*, 33 (1931), pp. 231-260 and IfK, *Russische Arbeiten zur Wirtschaftsforschung* Sonderheft 12 (Berlin, 1929).

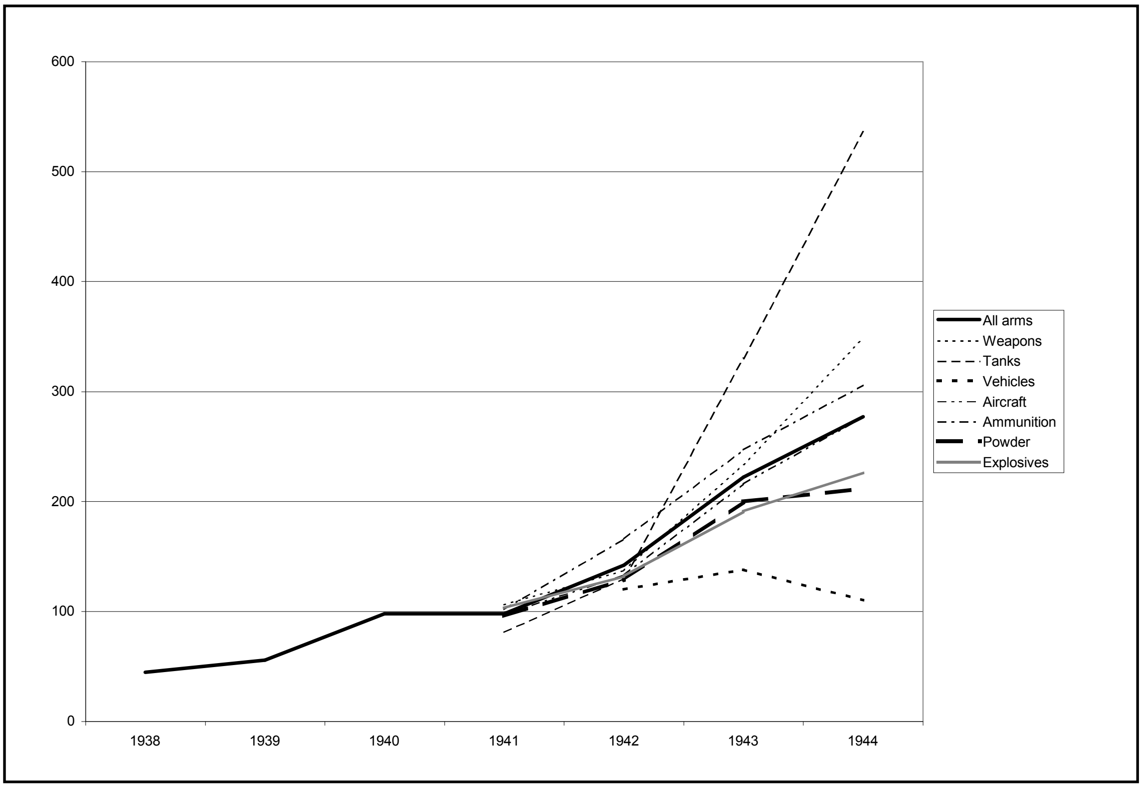
52 R. Wagenfuehr, Die Konjunkturtheorie in Russland (Jena, 1929), pp. 125-130.

53 Bundesarchiv Lichterfelde 25.01 Reichsbank 6593 No. 124 DIW, 'Die Deutsche Industrieproduktion im Kriege und ihre Messung' (June 1942).

and an enthusiast for modern methods of graphical presentation. In one case, at least, Kehrl's commitment to the Institute's barometer system had cost him dearly in business terms[54]. Nevertheless, Kehrl remained an enthusiast for economic statistics and indicators. More importantly, Kehrl was also a fanatical loyalist of the Nazi party and during the 1940s, he was to emerge as one of the key organizers of the German war economy. Indeed, recent research credits Kehrl, rather than Speer, as the organizing spirit behind the rationalization of Germany's wartime planning[55]. Kehrl, in turn, referred to the DIW and Wagenfuehr's team as his 'secret weapon'[56].

The most well-known product of this alliance between Konjunkturforschung and wartime planning was an index summarizing the output of armaments by German industry. This was a classic exercise in the art of index number construction. The aim was to weigh up the various elements of the war effort to gain some general idea of the expansion of armaments production as a whole. In political terms, the aim was to celebrate the achievements of the Speer Ministry and to help to bolster domestic morale. Technically speaking, Wagenfuehr's problem was to find a common denominator for the hundreds of different weapons systems and to take account of the constantly shifting program of armaments production. The result is one of the icon's of German economic history, with its dramatic upturn after the crisis winter of 1941-1942 exaggerated by the deliberate choice of base period. If one reflects on the mood of optimism and internationalism, in which the technique of index numbers first flourished in the late 19th century, Wagenfuehr's sophisticated measure of the aggregate destructive power of the Nazi regime, also stands as a sad testament to the perversity of the 20th century.

Figure 5: Wagenfuehr's Armaments Index and Components (Jan-Feb 1942=100)



54 DIW (IfK), *Das Deutsche Institut fuer Wirtschaftsforschung (IfK) 1925-1945 in der Erinnerung frueherer Mitarbeiter* (Berlin, 1966), p 111.
55 R.D. Mueller, *Der Manager der Kriegswirtschaft. Hans Kehrl* (Essen, 1999).
56 H. Kehrl, *Krisenmanager im Dritten Reich* (Duesseldorf, 1973), p. 319.

But index numbers were not the only contribution made by Konjunkturforschung to the Nazi war effort. The ultimate goal of the Planning Staff was to present Speer's Central Planning Board with a comprehensive overview of the relationship between the output of weapons and the inputs of about a dozen key inputs: labour, energy, transport capacity, steel, and scarce raw materials. This co-called Gesamtplan would give Kehrl the power he craved: to define the parameters of all military-economic decisions. It would also realize a long-held dream of Wagemann's Institute, as indicated by the following valedictory passage from a speech given by Wagemann in the Stalingrad-autumn of 1942:

> 'Once we have assembled a substantial collection of balances for raw materials and manufactures, then we will have made an important step towards a better understanding of the total circular flow (Gesamtkreislauf) of the German economy in its concrete form. The most important principle of economic research, as practised by my Institute, has always been that the economy is a total process and that it is therefore of fundamental importance to understand the interrelationship of individual phenomena. In the midst of the war, we are approaching this goal with giant strides. More than ever we are working to compile a total account of the national economy (Gesamtbilanz der Volkswirtschaft). Only then can planning of the economy - whether it be the control of the labour force, or raw material consumption, or stock movements, investment, consumption or trade - really begin.'[57]

The Gesamtplan came too late to have much effect on the practical work of Speer's Ministry. After the war, however, Wagenfuehr did not hesitate to place Kehrl's concept in the context of pre-war experiments in macroeconomic accounting[58]. In particular, he cited: Leontief's Input-Output scheme; the little known macroeconomic model prepared by the German engineer-economist Ferdinand Gruenig on the basis of IfK data[59]; and the Soviet efforts in national accounting, which Wagenfuehr and Leontief had both reviewed in the 1920s. The macroeconomics, which had underpinned the work of Konjunkturforschung since the 1920s, thus found an intellectual extension in the most advanced attempts to control the Nazi war economy.

5. Conclusion

As has been argued elsewhere in this collection, the barometer program of the interwar period derived a large part of their legitimacy from the importation into economics of mathematical techniques developed in the biometric sciences. Certainly, the Berlin Institute participated in this fashion. However, another way of giving solidity to business-cycle indicators was to root them in an aggregative, macroeconomic accounting framework. In Britain and France, this was not to occur until after World War II, and even then, relations between the two approaches remained tense. The central aim of this article has been to show how, in Germany, the development of the program of business-cycle indicators went hand in hand with the elaboration of an aggregative conception of the economy. In Germany, the two processes should be thought of as mutually reinforcing and originating at an early date in the 1920s. It was this intellectual synthesis that gave Konjunkturforschung its unusual longevity. The shock of the Great Depression did not call into question the value of the macroeconomic framework of analysis that had begun to be elaborated in the 1920s. In this sense, the history of the Berlin Institute resembles the development of the NBER rather more than the Harvard Committee. This interpretation of Weimar's Konjunkturforschung as a synthesis of business-cycle research with macroeconomic accounting also implies that we cannot stop our narrative with the Great Depression. After 1933, the advocates of Konjunkturforschung adapted and developed their instruments to serve the needs of Hitler's terroristic dictatorship. For Wagemann and his staff, the final years of the war brought the culmination of two decades of pioneering empirical macroeconomics. It was a remarkable achievement, but one it was convenient to forget.

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57 Imperial War Museum Sp Film 18 3038/49 Sc. 434 No. 61, p. 6.

58 DIW (IfK), *Das Deutsche Institut fuer Wirtschaftsforschung (IfK) 1925-1945 in der Erinnerung frueherer Mitarbeiter* (Berlin, 1966), p. 113.

59 On Gruenig and his French audience see F. Fourquet, *Les Comptes de la Puissance* (Paris, 1980), pp. 20-22 & 392-394.